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ADMINISTERING ELECTRIC SHOCK FOR INACCURACY IN CONTINUOUS MULTIPLE-CHOICE REACTIONS [1]

BY CARL N. REXROAD

Ohio State University

Since Yerkes made his studies of the effect of administering an electric shock to the dancing mouse for its wrong choices, his procedure has been rather generally adopted by animal investigators. Comparable studies of the effect of shock used as punishment on human beings suggest a field as yet scarcely touched. The investigation here reported was undertaken to determine this effect in one type of human performance.

Electric shock becomes punishment when it is used either for slowness or for inaccuracy. Yohanson (1) studied the effect of punishment of this nature for slow reactions in simple reaction-time experiments. He found the average reaction-time to be lowered 16 per cent., a lowering which was 14 times the P.E., when the shock was given. This study is of special significance since the reaction-time was previously considered as approximating the physiological limit. It would be of scientific value to study the effect of shocking for slowness in more complex types of performance. The apparatus and procedure used in this investigation would readily permit such a study. The present investigation was limited, however, after some preliminary experimentation, to a study of the effect of punishment upon inaccuracy.

[1] An abstract of the dissertation accepted by the Faculty of Yale University in partial fulfillment of the requirements for the doctorate in philosophy.

If a subject is to be punished for inaccuracy, the task set him must obviously involve choice. Five general types of experiment making this sort of a demand on the subject have been employed in psychological laboratories. These are maze experiments (including the pencil type), card-sorting, form-substitution, cancellation, and experiments using an apparatus for multiple-choice reactions. Hansen (2) has recently given an historical resumé of these types of experiment and pointed out their common characteristics. The type employing a special apparatus has the advantage for this investigation since it affords the possibility of delivering automatically the shock.

Apparatus

The simple apparatus used extensively since Wundt for measuring discriminative choice (Donders B or C) reaction-time might be employed. Such apparatus must, however, be set for each presentation. This involves a great deal of labor and time lost to the experimenter. Furthermore, the task set by such apparatus obtains only a fluctuating interest and attention from the subject. This would introduce into the experiment an undesirable variable. A continuous multiple-choice apparatus does not have these disadvantages, and is, therefore, most suitable for a study of the effect of punishment for inaccuracy. Various forms of continuous multiple-choice apparatus have been reported; but none was found entirely satisfactory for the present study. One was devised by the writer and reported in this Journal (1925, *8*, 325–336).

As here used, this apparatus presents five colors, red (R), white (W), green (G), orange (O), and blue (B), in random succession on a small piece of ground glass. The subject's reactions are made by pressing one of five keys of a five-finger keyboard. When the response is correct, the color next in order appears and the reaction is registered on an electrically operated Veeder counter. When the reaction is incorrect, the color remains but registration is made on another Veeder counter. Any key may be made 'correct' for any color.

The particular correspondence between keys and colors for a particular setting will be referred to as a code.

When punishment is to be given, the closing of the incorrect key also activates a relay. One lead from an inductorium is taken through the contact points of this relay to a small plate strapped firmly against the palm of the subject's left hand. The other lead is taken to a similar plate held against the back of the hand, about an inch behind the knuckles of the middle and fore fingers. The shock is felt through the latter plate.

Procedure

In order that any difference in results with and without shock may be ascribed to the shock, other factors that influence speed and accuracy of reaction must be kept constant or be balanced out. Such factors have been fairly well determined in numerous investigations. Summaries of these have been made by Wundt (3), Ladd and Woodworth (4), Jastrow (5), Henmon (6), and others. In view of these summaries all that need be mentioned here is the method of dealing with the factors.

Many of these factors remain constant when the same apparatus is used with and without punishment. In this class are the nature, size and intensity of the stimulus, the number of diverse stimuli, the amount of difference between the various stimuli, and the nature and amount of reaction to be made. Some factors may be kept constant by care on the part of the experimenter. The brightness of the general field was kept constant by using gray cardboard around the stimulus patch and by keeping the general illumination constant. This cardboard also served as a screen between the subject and apparatus, thus removing to a large extent the distracting influence of the apparatus.

For still other factors the experimenter can approximate constancy by adopting a rigid routine. The routine for a subject's first trial was as follows:

1. A typewritten copy of instructions was given the subject. These instructions, which were formulated after some

preliminary investigations, could be read as often as desired, but no questions concerning them were answered and no information regarding the particular code to be used was given.

2. The stimuli were shown in the manner in which they were to appear; except that they were caused to change by the experimenter's closing of his key for that purpose.

3. The subject was allowed to try the keyboard in order to familiarize himself with the placing of his fingers, the amount of key stroke required, *etc.*

4. If the subject was to receive shock, the contact plates were strapped to his left hand and he was told that he would receive a shock for errors as defined in his instructions. A few shocks were then given that the subject might know how much of a shock to expect and also that the intensity might be adjusted to a given subject. This adjustment for each subject was required because of the large individual differences in sensitivity to shock. The criterion for equality for various subjects was the flexing of the middle and fore fingers. The actual intensity required for this varied considerably; but it usually elicited the report, "That's about as much as I can stand."

5. All parts of the apparatus, except the circuits for the subject's keys and the shutter which kept the stimuli from being seen, were set in operation.

6. The subject was asked if he would like to go over his instructions again. When he reported that he was ready, the experimenter reminded him, "Five colors with one key for each. Remember that your score is the number of changes you make minus the number of errors."

7. The remaining circuits were closed, the experimenter simultaneously starting a stop watch and saying, "Go."

8. Readings of the two counters were taken each half minute.

9. At the end of the trial, subjects in part A (see below) were given their own scores, but not those of others, while subjects in parts *B* and *C* received both their own scores and those of others.

In subsequent trials steps 1, 2, and 3 were omitted and in parts *B* and *C* step 6 also.

The use of step 2 equalized the amount of foreknowledge of the stimuli. The arbitrary scoring mentioned in the instructions and again in step 6 tended to cause all individuals to distribute their efforts between speed and accuracy in the same manner. Mentioning the score just before the beginning of each trial and giving the actual score at the end of the trials tended to keep effort at a constantly high level. Also the nature of the task itself tended to call out a constantly high degree of effort.

The remaining factors are not directly under the experimenter's control; but they may be dealt with in special ways. The effects from individual difference and daily fluctuation were theoretically balanced out by the number of subjects used and the number of trials given. The effect from practice was dealt with by dividing the investigation into parts. Preliminary studies had indicated that after 2½ minutes, with a particular code, the rate of improvement decreased very sharply, and also that when a number of codes were given, each for 2½ minutes, there was considerable improvement from the first to the last. Thus practice was shown to result in two sorts of improvement, improvement in reacting to a specific code and improvement in ability to learn any code. The former will hereafter be referred to as 'specific' learning and the latter as 'general.' The investigation was so planned that a determination of the effect of punishment might be made during 'specific' learning, during 'general' learning, and during little or no evident learning.

Three groups of subjects, designated *A*, *B*, and *C*, were used. Each individual in group *A* was given ten codes, each for 2½ minutes with one-minute intervals. The ten codes, in the order used, are as follows, the color given first being correct for the thumb, the one given second for the fore finger, *etc.*:

Code	1.	O	B	W	R	G	6.	G	R	W	B	O
	2.	R	O	G	W	B	7.	W	O	R	G	B
	3.	W	R	B	G	O	8.	R	B	O	W	G
	4.	G	W	O	B	R	9.	O	G	B	R	W
	5.	B	G	R	O	W	10.	B	W	G	O	R

Each subject in groups *B* and *C* was given 24 fifteen-minute trials on successive school days. The first four trials were considered practice trials. Group *B* received code 1 throughout, while group *C* was given codes 6–9 for the practice trials and codes 1–5 in rotation for the test trials. Thus, group *A* performed while both 'specific' and 'general' learning were proceeding rapidly, group *B* made test records after improvement had practically ceased, and group *C* made test records after 'general' learning had practically ceased; but, since each trial involved the learning of a new or forgotten code, these records showed 'specific' learning during the first $2\frac{1}{2}$ minutes. This part of the *C* record will be designated *Ca* and the last part *Cb*, in which the conditions are practically the same as for group *B*. The effect of punishment during 'specific' learning is to be determined from the results of part *Ca*, during 'general' learning from the results of part *A* in contrast with part *Ca*, and during little or no evident learning from parts *B* and *Cb*.

Group *A* was composed of sixty individuals, all taking courses in elementary psychology. Groups *B* and *C* were composed of 8 and 6 individuals respectively, all graduate students or instructors in psychology. Punishment was administered to alternate individuals of group *A* and to individuals of groups *B* and *C* on alternate days, half of each group receiving punishment on the odd trials and half on the even trials.

Preliminary investigation had shown that the codes differed in difficulty, and thus that another variable was being introduced by using various codes in parts *A* and *C*. This merely required that the same codes be used in the same order for both the punished and the unpunished individuals of group *A*, and that in part *C* each code be used as often with punishment as without.

Results

The results are given statistically in numbered tables and figures. In the tables the horizontal rows have been designated at the left with symbols or abbreviations. Thus C

is used for the number of correct responses, E for the number of errors, o when no punishment was given, and p when punishment was given. Accordingly, Av. Co., in Table 1–*A*,

TABLE 1–*A*

Trials	1	2	3	4	5	6	7	8	9	10	Av.
Av. Co.	57.1	74.9	79.2	80.6	86.7	86.3	100.6	95.4	91.9	96.8	84.95
S. D.	19.5	17.3	21.6	21.2	22.0	25.1	24.9	24.1	23.1	25.5	22.43
Av. Cp	58.8	72.5	81.7	84.1	92.3	90.7	102.9	92.3	97.7	95.6	86.86
S. D.	20.2	18.3	18.5	20.7	17.9	20.8	15.8	21.2	19.2	19.1	19.17
R dif	.1	.1	.1	.1	.2	.1	.1	.1	.2	.0	.06
Per cent. iC	3.0	−3.2	3.2	4.3	6.5	5.1	2.3	−3.2	6.3	−1.2	2.29
Av. Eo	53.1	50.7	47.1	49.6	41.9	43.6	35.7	38.9	39.3	41.4	44.13
S. D.	22.7	21.6	20.8	21.7	22.2	19.8	21.1	20.1	20.3	23.2	21.35
Av. Ep	51.2	51.4	45.1	40.3	36.0	35.9	26.5	33.8	28.2	31.8	38.02
S. D.	24.1	23.1	22.2	17.1	22.7	20.2	13.6	18.8	14.8	17.9	19.45
R dif.	.1	.0	.1	.3	.2	.3	.4	.2	.4	.4	.21
Per cent. iA	6.4	−4.7	7.2	22.1	19.3	24.1	27.4	10.2	9.1	22.2	15.73

is the average number of correct responses made by individuals without punishment. R dif. is used to indicate the reliability of the difference between records with punishment and without and is obtained by dividing the obtained difference by the sigma of the difference. Per cent. iC indicates the percentage increase in number of correct responses with punishment, and Per cent. iA the same for accuracy. Per cent. iA is in calculation the percentage decrease in inaccuracy, inaccuracy being the ratio of errors to correct responses. In case of decrease in either the number of correct responses or in accuracy the values for Per cent. iC and Per cent. iA become negative.

The results for part *A* are shown in Tables 1–*A* and 2–*A*, the first giving the results for the punished and unpunished halves of the group when the data are tabulated by trials ($2\frac{1}{2}$ minutes each), and Table 2–*A* showing the results when the same data are tabulated by half-minutes, the records of each half-minute of all trials of all subjects being grouped.

TABLE 2–*A*

½ min.	1	2	3	4	5	Av.
Av. Co.	13.68	15.49	17.22	18.28	20.27	16.99
S. D.	4.76	5.53	5.92	6.22	6.35	6.20
Av. Cp.	13.94	15.84	17.70	18.78	20.58	17.37
S. D.	4.66	5.12	5.52	5.32	5.52	5.73
R dif.	.14	.05	.06	.06	.04	.05
Per cent. iC	1.9	2.3	2.8	2.7	1.5	2.29
Av. Eo.	12.86	9.79	8.51	6.89	6.08	8.82
S. D.	4.82	5.16	5.37	5.29	5.39	5.74
Av. Ep.	12.07	8.42	7.02	5.62	4.89	7.60
S. D.	4.79	5.16	5.20	5.19	5.00	5.68
R dif.	.12	.19	.20	.18	.17	.15
Per cent. iA.	8.9	15.9	19.7	20.6	20.8	15.73

It will be noted from these tables that the punished individuals average 2.29 per cent. more correct responses and 15.73 per cent. greater accuracy than the unpunished individuals. Punishment, then, had the effect of increasing performance. But it will be also noted that the values of R dif. are small. Ordinarily, these values are treated as measures of reliability. But they primarily indicate the relative effect of a controlled variable and of one or more uncontrolled variables. If the values are high (3 or more), they show that the controlled factor has been the dominant factor in determining the results and if low that the uncontrolled factors have played a large part in the results. The more uncontrolled factors enter into the results, the greater is the chance that the difference in results with and without the presence of the controlled variable may be due to the uncontrolled rather than to the controlled factor. Hence the numerical values of this measure may be said in a secondary way to indicate reliability. A low value, however, does not necessarily indicate unreliability. It merely leaves the burden of showing reliability to other considerations. In the present case the controlled variable was the electric shock and the uncontrolled variable the individual differences. Since the values of R dif. are low, it is safe to

say that punishment does not affect performance nearly so much as individual difference. Also, since the values of R dif. are low, it is necessary to show by other means that the difference between the two groups can reliably be said to be due to the punishment.

In this connection it will be noted from Table 1–*A* that on the first two trials the two groups were practically equal in number of correct responses and in accuracy, but that on later trials the difference between the two groups, particularly in accuracy, becomes progressively greater. Table 2–*A* shows also this progressively increased difference from the first half-minute to the end of the trials. These facts tend to show that the natural or initial ability of the two groups was equal, but that, during the experiment itself, some factor entered to cause a divergence in performance. If the procedure and technique of the experiment were sound, then this factor must have been the punishment.

Another group of facts pointing to reliability of the obtained differences does now show in the tables. As soon as records were secured from ten individuals in each group, the results were tabulated. The first ten with punishment made 1.31 per cent. more correct responses and were 8.14 per cent. more accurate than the first ten without punishment; the second ten with punishment made 0.39 per cent. more correct responses and were 25.13 per cent. more accurate than the second ten without, and the third ten with punishment made 5.25 per cent. more correct responses and were 15.56 per cent. more accurate than the third ten without. In addition a group of ten with punishment made in a preliminary study 2.71 per cent. more correct responses and were 18.62 per cent. more accurate than a group of ten without punishment. It is hardly probable that the results would have been so consistent by chance. In fact, another set of compared groups would have to differ from the average of these four groups by 1.5 sigma if the group with punishment would not show more correct responses than the group without and by 2.7 sigma if the punished group would not show more accuracy than the unpunished group. The

probability that this would happen is lessened by the fact that the groups used give a normal distribution in their scatter, *i.e.*, a random sampling was taken. As an example of the distribution obtained, the following facts might be given. The thirty unpunished individuals, when grouped on the basis of the number of correct responses made on the first trial—20 to 29, 30 to 39, *etc.*, scattered as follows; 2, 4, 5, 7, 5, 4, 2, 1. Not all distributions approximated the normal as nearly as this, but all showed the tendency toward normal distribution.

It might be pointed out in this connection that taking more subjects would not have affected to any marked extent the values of R dif. This value would be changed only if the difference between the two groups increased markedly or if the individuals in the two groups decreased markedly in their amount of scatter. The facts presented in the above paragraph tend to show that neither of these changes would have been likely to result.

Another significant fact is that the amount of scatter in the punished group (as shown by the sigmas) becomes progressively less in respect to the amount shown by the unpunished group. This means that the better end-achievement of the punished group is largely due to a very rapid improvement of the individuals who start in poorly. This is to be expected, since the poorer individuals receive more shock and since shock would most affect incentive and comprehension of the task in those individuals in whom these factors are low.

In view of the above considerations, it seems safe to conclude that the differences between the two groups were due to punishment and to say that punishment given under the conditions of part *A* increases the number of correct responses by 2.29 per cent. and the accuracy by 15.73 per cent. on the average.

The results for part *B* are shown in Table 1–*B* and Fig. 1–*B*. In the figure the data from all subjects have been combined. These average results may be considered first.

TABLE 1–*B*

Subjects	1	2	3	4	5	6	7	8	Av.
Av. Co.....	31.40	29.40	34.28	31.83	27.10	33.66	27.97	34.49	32.54
S. D.....	3.74	2.22	2.32	2.03	4.70	4.13	3.01	4.47	4.70
Av. Cp.....	29.69	29.33	33.28	29.65	36.77	33.00	27.04	32.14	31.79
S. D.....	3.56	2.53	3.64	3.21	5.35	4.44	4.51	4.10	4.95
R dif.....	.33	.02	.24	.57	.04	.11	.17	.39	.11
Per cent. iC...	−4.51	−.02	−2.92	−6.86	−.89	−1.96	−.32	−4.39	−2.34
Av. Eo.....	1.19	3.43	4.06	.74	1.79	.36	.35	3.50	2.01
S. D.....	1.50	2.36	2.56	.54	1.07	.58	.63	1.99	2.19
Av. Ep.....	.84	3.13	3.70	.31	1.54	.39	.48	2.77	1.67
S. D.....	1.06	2.41	3.67	.48	1.00	.61	.72	1.79	2.22
R dif.....	.09	.30	.08	.60	.17	.04	.13	.46	.11
Per cent. iA...	25.42	9.51	6.14	55.00	13.18	−10.53	−41.77	30.42	14.91

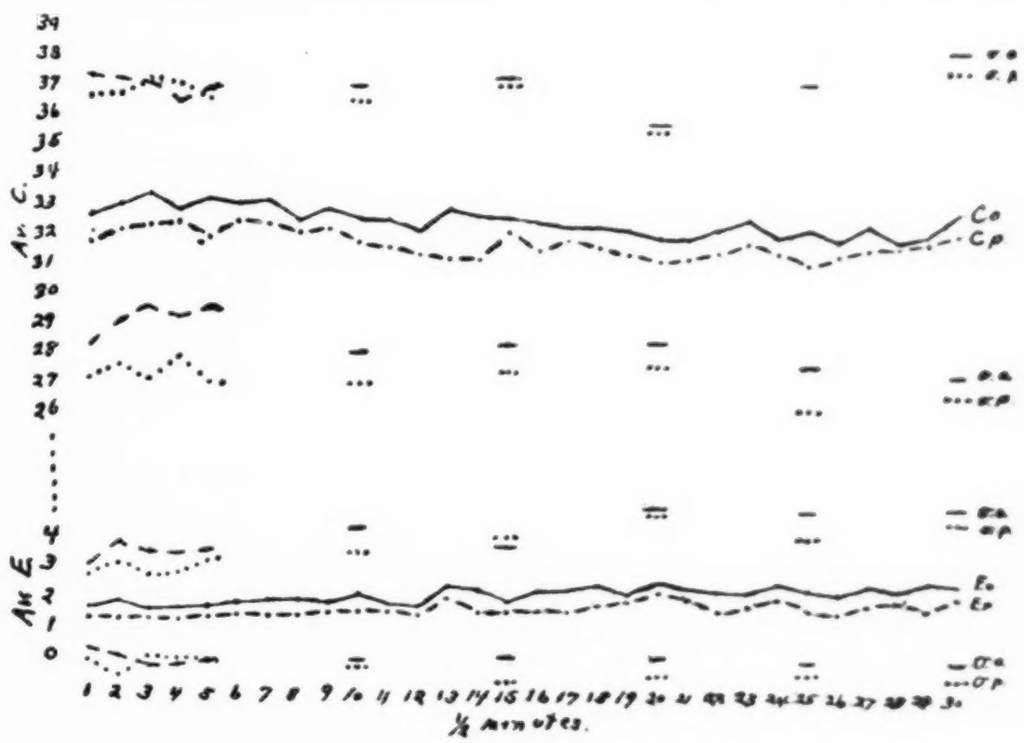

FIG. 1–*B*

The significant fact shown in Fig. 1–*B* is that even with large sigmas the lines for the two conditions do not cross. This confirms in a fairly positive manner what was said in discussing the results of part *A*. Although the difference due to punishment is slight, it is, nevertheless, so constant that

one can scarcely doubt that it is a real and significant difference in spite of the fact that the value for the reliability measure is only 0.11.

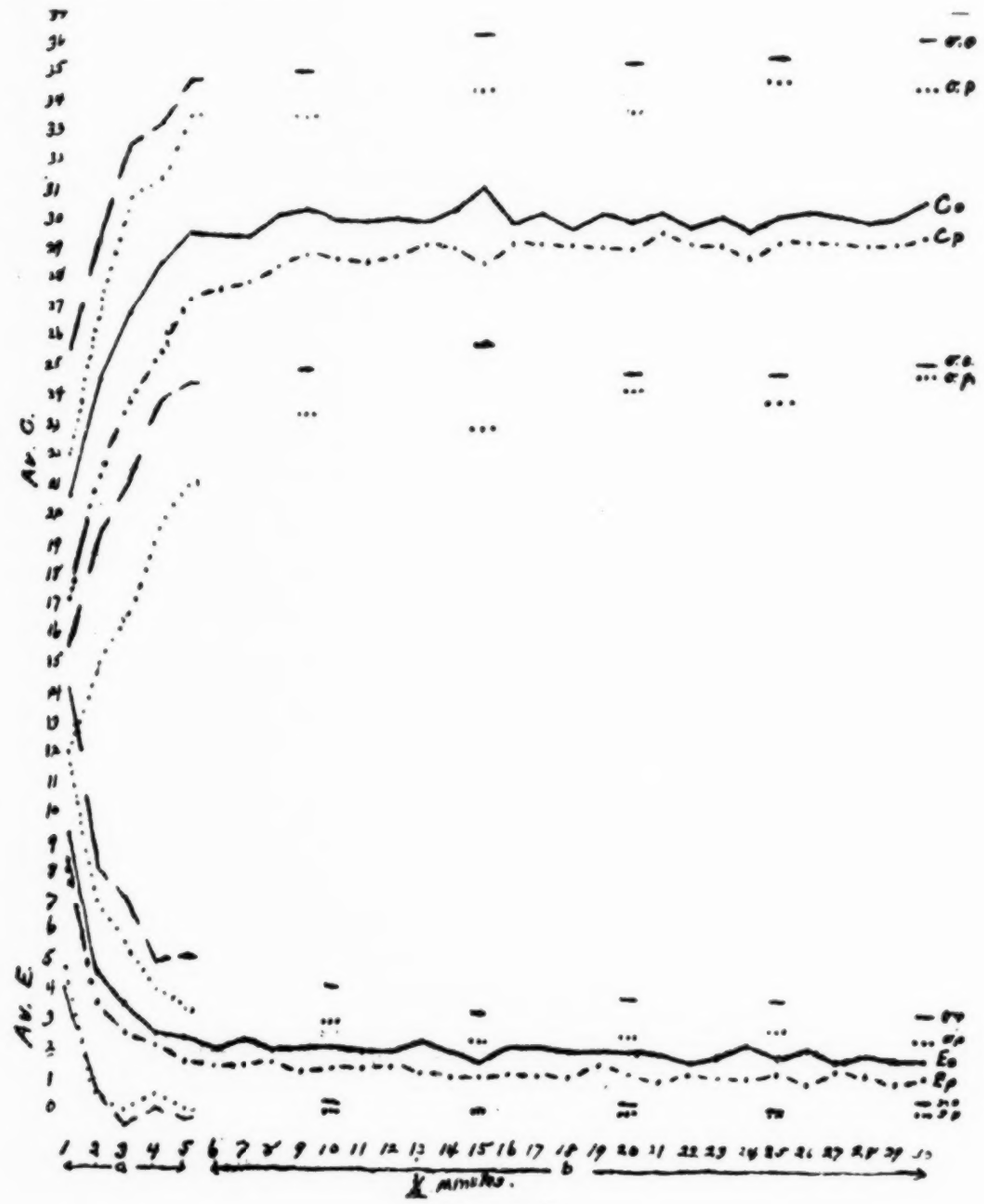

FIG. 1–C

The average decrease of 2.34 per cent. in number of correct responses is seen from Table 1–*B* to be due to slight decreases by every subject. If it be considered that a subject must either increase or decrease in number of responses with punishment, then the chance that all eight individuals would show a decrease is only 1 in 264 ($\frac{1}{2}$ to the eighth power).

If, however, the amounts of decrease, expressed here in percentages, are considered, then another subject would have to differ from these by 1.1 sigma if he were not to show a decrease.

The average increase in accuracy of 14.91 per cent. is seen to be due to large increases by six subjects, which offset the decrease in two subjects. These two subjects had physical handicaps such that all their responses were necessarily made with the forefinger. Whether this difference in the form of response in any way accounts for the difference in the effect of punishment on their accuracy cannot be stated. It will be noted that they make few errors either with or without punishment. Whatever the explanation of their difference, the chance that six of eight individuals would show an increase in accuracy is 1 in 16, or, considering the percentage amounts of the differences, another individual would have to differ from these by 0.8 sigma if he were not to show more accuracy with punishment than without.

The condition with reference to the stage of learning in part *Cb* is the same as for part *B* and the results here are exactly parallel. The results are shown in Fig. 1–*C* as they were shown for part *B* in Fig. 1–*B*, the figure for the last 12½ minutes being *Cb* data. Table 1–*Cb* was derived in the same manner as Table 1–*B*.

TABLE 1–*Cb*

Subjects	1	2	3	4	5	6	Av.
Av. Co.....	35.6	23.2	32.4	33.6	30.1	26.3	30.2
S. D.....	2.81	3.83	3.09	2.56	2.62	4.72	5.44
Av. Cp.....	33.4	24.2	32.9	32.7	28.8	23.0	29.2
S. D.....	2.77	3.06	2.19	3.57	3.39	4.54	5.36
R dif.....	.57	.20	.13	.21	.36	.50	.13
Per cent. iC.....	−6.2	4.3	1.5	−2.7	−4.3	−12.6	−3.4
Av. Eo.....	2.28	.48	1.34	1.92	2.89	3.22	2.02
S. D.....	1.69	.72	1.11	1.50	2.08	1.98	1.84
Av. Ep.....	1.05	.39	.90	1.92	2.59	1.22	1.36
S. D.....	1.15	.58	.93	1.54	1.77	1.21	1.50
R dif.....	.60	.10	.30	.00	.11	.86	.28
Per cent. iA.....	51.0	22.1	33.9	−2.7	6.3	56.7	30.4

From Table 1–*Cb* it will be noted that four of the six subjects showed a decrease in number of correct responses with punishment, and that five of the six showed an increase in accuracy. Of the fourteen individuals in parts *B* and *Cb*, twelve decreased in number of correct responses. The possibility that this should happen by chance is only 1 in 1024. Of the fourteen subjects eleven increased in accuracy. This represents one chance in 256.

These results from parts *B* and *Cb* have shown without much doubt that punishment has the effect of decreasing the number of correct responses and increasing accuracy after 'general' and 'specific' learning are approximately complete.

The results for part *Ca*, which were secured after 'general' improvement had practically ceased, but during the period of rapid 'specific' learning, are shown in the first five half-minutes of Fig. 1–*C* and in Table 1–*Ca*. In this part the

TABLE 1–*Ca*

Subjects	1	2	3	4	5	6	Av.
Av. Co.....	147.4	102.1	136.0	134.3	129.9	129.1	129.8
S. D.....	16.3	14.8	18.5	13.0	13.6	16.9	15.5
Av. Cp.....	120.8	88.9	133.1	125.9	114.7	107.0	115.1
S. D.....	29.3	12.2	21.1	28.1	18.4	18.6	21.3
R dif.....	.6	.3	.0	.0	.5	.8	.6
Per cent. iC.....	−18.1	−12.9	−2.1	−6.3	−11.7	−17.1	−11.4
Av. Eo.....	43.8	18.7	13.6	19.1	20.3	19.2	22.4
S. D.....	22.2	10.5	4.1	7.1	7.2	7.1	9.7
Av. Ep.....	27.9	15.5	13.9	19.0	25.3	13.2	19.1
S. D.....	15.0	6.1	6.5	7.5	6.5	2.4	7.3
R dif.....	.6	.3	.0	.0	.5	.8	.3
Per cent. iA.....	22.3	4.8	−4.4	−6.1	−41.0	17.1	3.8

number of correct responses decreased 11.4 per cent. on the average, all subjects showing some decrease. In the average results there was an increase in accuracy of 3.8 per cent., three subjects showing an increase and three a decrease, those decreasing making a larger relative decrease.

Thus punishment during rapid 'specific' learning produces

a decrease in the number of correct responses with either a small effect on accuracy or an effect that varies with different subjects.

These results of part *Ca*, when compared with the results of part *A*, should give the effect of punishment on 'general' learning. The results during 'specific' learning have shown a decrease in the number of correct responses, while the results during combined 'specific' and 'general' learning have shown an increase. The effect on 'general' learning is, therefore, to increase the number of correct responses. During 'specific' learning there is little average effect on accuracy but during combined 'specific' and 'general' learning there is a marked increase in accuracy. The effect of punishment during 'general' learning is, then, to increase accuracy.

Discussion and Conclusions

The explanation for the varying effects of punishment under the three different conditions of the experiment may be found in the reports of the subjects or by considering on a priori grounds what effects might be expected. In this manner the conclusion is reached that punishment does not have a single effect but, instead, it has three effects. Punishment may act as an incentive, it may have an instructive effect, or it may have a disruptive or distractive influence. The first two would act together to give a better performance, the last would tend to make a poorer performance, particularly in speed. In animal investigations the instructive effect is large, there being but one other manner of informing the animal that it is making a wrong choice. The disruptive effect is also present; but does not show in the results of some investigations, since in these the number of trials required for learning is taken as the sole measure of efficiency.

Applying this analysis to the results of the present investigation will tend to confirm the analysis and at the same time will explain the divergence between results of the separate parts. 'General' improvement is most reasonably due to the subject's becoming familiar with the requirements of the task and to his discovering an efficient method for

rapid 'specific' learning. It seems impossible to formulate instructions such that freshmen and sophomores, who have had no experience as subjects, will understand completely the requirements of their task. Administration of shock for errors will define errors in an unmistakable manner, *i.e.*, punishment will have an instructive value additional to that of verbal instructions. Even if these subjects clearly understood their task, they lack the experience which would cause them to approach it with a systematic method or plan. But with sufficient incentive they will attempt in the course of the experiment to devise a method that will make them more efficient. At the end of the ten trials in part *A* each subject was asked whether he had used a particular method and when he had adopted that method. All but one of the punished individuals reported a definite method, while nine of the unpunished individuals could report none. It is fair, then, to hold that punishment acted as an incentive for discovering a method. Thus in part *A* conditions were such that the instructive and incentive effect of punishment would be large and the results show by the increase in speed and accuracy that this was the case.

The method referred to above differed considerably with various subjects but agreed in one respect. All methods involved a scheme for a verbal learning of the code. This involved, for most subjects, linking the name of a color with some designation of the key or reaction to be made to it, *e.g.*, for code 10 the verbal response when blue was presented might be, 'Blue-one,' 'Blue-thumb,' 'Blue-left,' or 'Blue-outside.' These verbal responses were often made aloud, so the experimenter did not need to depend entirely on the subject's report. Some subjects used as their method another scheme. They would not name any color until the one for the thumb appeared, then the one for the forefinger, *etc.*, so that the colors were learned in a series corresponding with the order of the keys. In these cases, if the color for the little finger appeared, all five colors were named in series before a finger response was given.

In parts *B* and *Cb* the effect of punishment showed itself

in greater accuracy with less speed. The incentive effect accounts quite readily for the greater accuracy while the disruptive effect accounts for the loss in number of correct responses. Besides the immediate disruptive effect there was a secondary sort of disruption reported by most subjects. After practice with a code the interpolated verbal responses tended to drop out, the finger response being made immediately (involuntarily) to the stimulus. Punishment tends to halt or disrupt this change, since an error is more likely to occur in this way. The subject, when punished for his errors, comes to distrust his fingers and reverts to the surer but slower method.

In part *Ca* punishment showed its effect in decreased speed without a compensating increase in accuracy. This means that the disruptive effect was more pronounced than in part *B* and the incentive effect less marked. The increase in disruptive effect is in accord with the principle that a disruption is more pronounced in the less well-established reactions. Subjects of group *C* reported time and again that, just as they were getting a code verbally learned, they would receive a shock and forget all they had learned. After the code was fairly well verbally learned, the shock would at most cause only a temporary forgetting. The incentive influence would not show in this part of the experiment even though it might be active, since quite obviously it is useless to try to avoid an error if one does not know specifically which responses are erroneous.

The fact that the results are so perfectly in accord with this analysis of the effects of punishment is an additional reason for considering the results reliable and is reason for believing the analysis to be correct.

The conclusions with respect to the effect of punishment in continuous multiple choice reactions may now be stated in summary form.

1. Punishment has three effects, disruptive, incentive and instructive.

2. The resultant of these three effects in no case offsets the effects from individual differences and daily fluctuations.

3. The instructive effect is in inverse proportion to the previous comprehension of the task, and consequently is not present after a certain amount of practice.

4. The incentive effect shows itself in the rapid adoption of a scheme or plan for learning any code and in greater care to avoid errors throughout the performance.

5. The disruptive effect is in inverse proportion to the thoroughness with which the habit is established, and consequently will offset the incentive effect during the learning of a code, but will be offset by the incentive effect after the code is learned.

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RESPONSE TO A SUDDEN UNEXPECTED STIMULUS

BY EDMUND JACOBSON

Few studies have been made, either in physiology or in psychology, of the excessive response which takes place after a sudden unexpected stimulus. Yet the involuntary start is familiar to everyone. It usually occurs after a sudden noise or other strong excitation while the individual is otherwise engaged. The movement, when extensive, generally includes a straightening of the trunk due to the erector spinæ; but jerks of the limbs or head are also common, and any external muscle-group may be involved. Often the disturbance is so slight that the only external evidence is a rapid contraction of the eyelids; although the individual may report organic sensations as of 'shock.' What is the explanation of this neuro-muscular excitation that follows a strong unexpected stimulus? It is evident that some of the movements are sensory adjustments to the new stimulus; *e.g.*, turning toward the sound with wide open eyes. But the jerks of the trunk or limbs are obviously not sensory adjustments, and they stand in need of further explanation. Lehmann (1) made a study of the effects on the pulse, respiration and volume of the arm produced by a loud unexpected organ note. Féré (2) registered contractions by means of a tambour placed on the muscles of the forearm or wrist, while at other times he used a hollow ball held in the hand. He concluded that the contractions were reflexes, which varied in amplitude with the intensity of the excitation, the excitability of the subject, and the unexpectedness of the stimulus. His studies were qualitative and obviously incomplete.

In 1908 I began an experimental study of the involuntary start in order to test an hypothesis which I have since then had to abandon. It is easy to observe that the start generally occurs when the individual is said, in lay terms, to be "lost

in thought." Accordingly it seemed plausible to suppose that, upon sudden interruption of the psychophysical thought-processes, the energy from the latter might be diverted to peripheral muscles. Preliminary tests lent a certain weight to this assumption. The subject was directed to attend to the details of a coin or to some simple but interesting picture. Since the movements to be recorded were, in part, directed upward and backward, a thread was attached to the back of the head, passing up and back at an angle of 45° to the vertical, through a small steel loop, and then vertically up to a broad tin writing-point, held in position upon the drum by a second thread. This second thread was continued by a short elastic cord, one end of which was clamped a little above the upper end of the drum. The kymograph was placed in a vertical position, its lower edge six inches or more above the head of the subject. In this way upward and backward movements, or the reverse, were very crudely recorded. Without seeking to generalize, it was found, at least at times, that during marked attention the start was strong (III. in Figs. 1, 2 and 3), while with mental passivity the start might be absent altogether (0 in same figures). We sought to distinguish, by advance instruction to that effect, an intermediate state of attention (I. in the same figures).

Since experimentation calls for frequent repetition of a phenomenon under like conditions, it was clear that no great progress could be made if the involuntary start should soon disappear. This unfortunate occurrence seemed probable, if, as I had previously believed, the start depended primarily upon unexpectedness. I discovered, however, that, under the conditions of the experiment, the stimulus could be repeated as often as ten to fifteen times during a period and the start would still occur, with some adaptation, to be sure, but not so much as to interfere with comparisons. The stimulus was a sudden loud noise, made by releasing one end of a strip of resilient wood (of dimensions somewhat greater than those of a meter-stick) clamped at the other end to a wooden table. The volume of sound was kept fairly constant by raising the free end to a determined height.

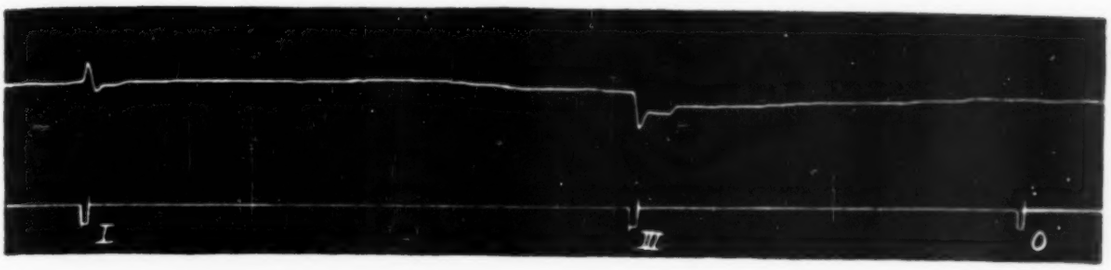

FIG. 1. Subj. 1

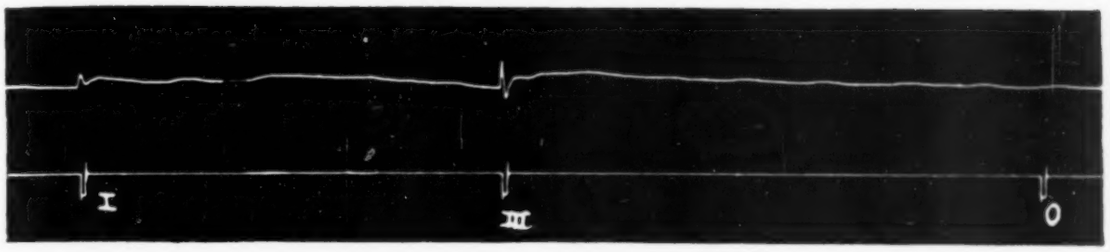

FIG. 2. Subj. 2

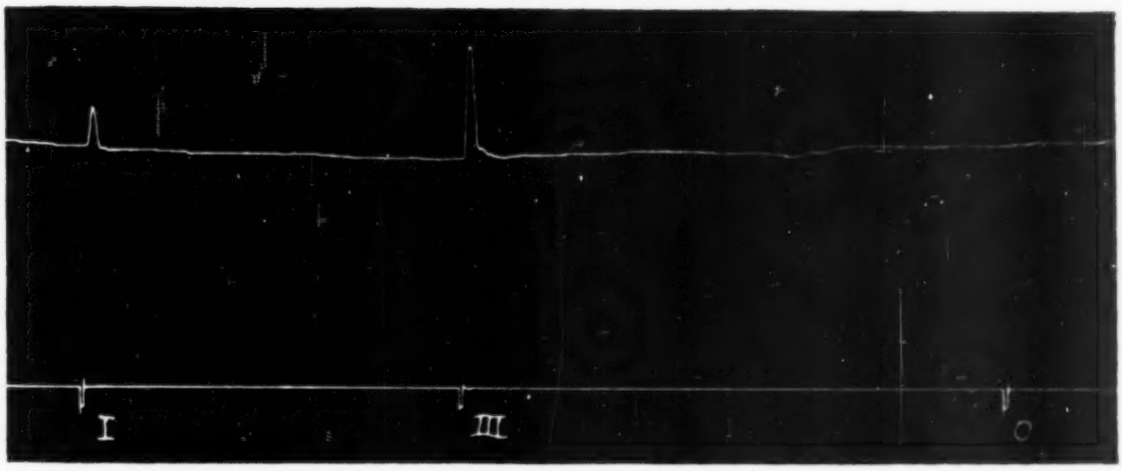

FIG. 3. Subj. 3

FIGS. 1, 2, 3. The upper line indicates movements of the head and trunk. Breaks in the lower line indicate time of the shock. At III. the subject was fully attentive; at O he was so far as possible inattentive, while at I. there was an intermediate degree of attention. The figures show the most marked start where attention was greatest, and no start during inattention.

Now upon closely watching the subjects when they were directed to be attentive, it was noted that the brows contracted, the eyes were fixed, the limbs slightly stiffened and the trunk tensely held. On the other hand, when directed to be inattentive, tension seemed to give way to relaxation. These observations gradually led me away from the hypothesis mentioned above. A simple explanation of the start seemed to be at hand. The muscles contracted with a jerk when the strong stimulus came because they were previously contracting, although in less degree, during attention. If there was no such previous contraction, as during inattention, no start took place. Evidently this simple explanation was more acceptable than my earlier complex hypothesis.

The truth of this explanation could be tested by producing, in some other way, a heightened peripheral contraction or tonus where the sudden noise might nevertheless produce a further contraction varying in amplitude with the tonus. The subject was accordingly directed to stiffen the muscles of the arms, legs, head and trunks; that is, to sit still while partially contracting his muscles. In other tests he was directed to relax his muscles as completely as possible. An intermediate condition was also directed, the muscles being held only slightly tense. These grades are respectively indicated by the numbers 3, 0, 1, in the figures. When the sound was made under these conditions, the subject while looking away gave the same involuntary start as before. Indeed, the contraction seemed in these preliminary tests very much greater with extreme muscular tenseness than with mild; while with relaxation there was no start and no shock, and the sound seemed to lose its irritating character (Figs. 4, 5 and 6).

In this way it was suggested that, in addition to the factors mentioned by other writers, the involuntary start depends upon the previous neuro-muscular tonus; and that, if there is a correlation between the interruption of thought-processes or of attention and the occurrence of a start, this is because the mental activity has a motor accompaniment.

The observations begun at Harvard were continued on

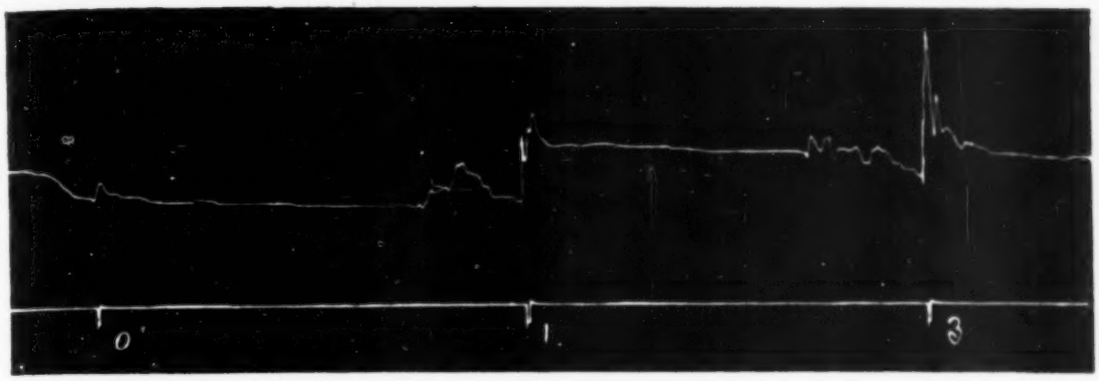

FIG. 4. Subj. 1

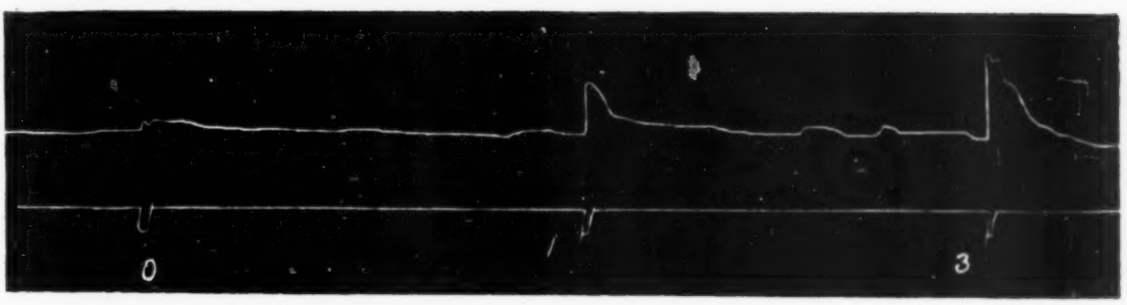

FIG. 5. Subj. 2

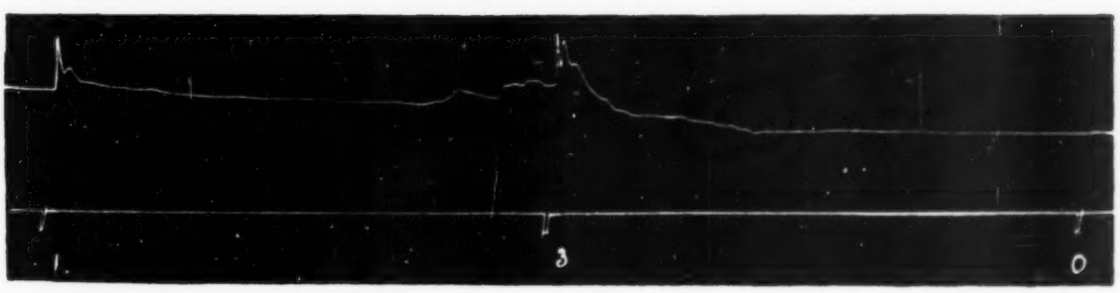

FIG. 6. Subj. 3

FIGS. 4, 5, 6. At 3 the subject was directed to contract generally the muscles of the limbs, trunk and head; at 0 he was relaxed, while at 1 there was an intermediate degree of general contraction. The most marked start is here shown where the preceding neuro-muscular tonus is highest, while with relaxation there is no start.

several visits to the Willard State Hospital of New York. They seemed too few in number, however, to permit more than a provisional generalization, particularly since marked exceptions to the rule often proved puzzling. In 1921–22, therefore, thanks to Professor Carr, the experiments were continued by Miss E. Lodor and myself at the University of Chicago, and later by Miss M. Miller who used a pistol to set off the start. The attempt was again made to record movements of the trunk, but in spite of attempted refinements, the graphs seemed crude and inadequate. Sommer's tri-dimensional analyser did not seem well adapted to our purposes; for we did not know in advance what parts would jerk or what directions they would take.

At this stage it seemed necessary, in order to get the large number of precise observations required for scientific generalizations, to restrict the study to the movements of some particular part of the organism. Thereupon the classical studies of the flexion reflex in physiology came to mind. It was evident that a quantitative inquiry into this reflex could be made in terms of speed and extent of movement, so that the influence of relaxation might be studied under more accurate conditions. If, in response to a sudden strong stimulus, the flexion reflex should be found to be diminished in the presence of general relaxation, this would agree with the results of the present article and tend to support its conclusions. These experiments on the flexion reflex are described below in the article by Miss Miller. The above discussion and results, although still to be regarded as provisional, meanwhile provide a larger setting for this work.

Summary.—The observations presented suggest that both the sensory and the motor responses of the organism to a sudden strong stimulus depend upon the preceding general state of muscular tonus; that the feeling of 'shock' is weak or absent and the start is wanting or minimal where the individual is extremely relaxed.

We do not here mean 'relaxation' in the popular sense. An individual may be 'relaxed,' in this sense, yet start vio-

lently at a sudden noise. Such partial relaxation may produce an apparent increase in the reflex because of a diminished contraction of muscles antagonistic to the movement of the start. This fact is also familiar in clinical tests of the knee-jerk, where the subject is requested to 'relax,' in the popular sense, in order to permit the jerk to appear. But in real and extreme relaxation the knee-jerk has been shown to be reduced or entirely absent (3), a result which harmonizes with the article which immediately follows.

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CHANGES IN THE RESPONSE TO ELECTRIC SHOCK PRODUCED BY VARYING MUSCULAR CONDITIONS[1]

BY MARGARET MILLER

The general problem of this experiment was to determine the effect of various conditions of muscular contraction and relaxation on the non-voluntary response to an electric shock. The stimulus was an induced current applied for a fraction of a second to the fingers, and the response measured was the upward jerk of the arm which followed. The shock was given without warning, and the subject was instructed not to attempt any voluntary control of his reaction. Graphic records of the reaction showed the extent of movement and the reaction-time.

The present paper deals with the first part of the experiment, in which the response made during extreme relaxation was compared with that obtained during ordinary rest upon a couch. The persons who served as subjects received preliminary training in inducing relaxation from Dr. Edmund Jacobson. At the end of this training they were able to assume and to maintain a state of general muscular relaxation which differed radically, in the judgment of subjects and experimenter, from the ordinary muscular condition.

Apparatus

The subject lay on a couch with the tips of two fingers of the right hand immersed in two cups of normal salt solution. The stimulating current was passed through this solution, producing ordinarily a quick withdrawal of the hand. For each individual a particular strength and duration of current were selected and kept the same throughout the experiment,

[1] The experiment was conducted in the Psychological Laboratory of the University of Chicago. The problem was suggested by Dr. Edmund Jacobson and the procedure carried through with his assistance and advice. The author is indebted to Professor Harvey A. Carr for much helpful criticism and many suggestions.

with exceptions to be noted later. The frequency did not vary for the several subjects.

The arm was strapped to a light movable frame arranged in such a way that the only movement permitted was upward from the elbow. This frame was connected with a lever which recorded the arm movement on a revolving drum. A signal magnet registered the beginning of the stimulus, and a tuning fork of 256 d.v. traced a time line.

The apparatus for giving the stimulus was designed to secure a current which could be controlled in strength, frequency, and duration.[2] The current from a storage battery was led through the primary coil of a small inductorium. It was interrupted 260 times a second by means of a commutator driven by a constant speed motor. In the circuit were a milliammeter and a slide rheostat, by means of which the strength of the primary current could be kept constant.

The current from the secondary coil of the inductorium was led through the solution in which the subject's fingers were immersed. This circuit was closed by an automatic sliding contact, a copper strip passing over a brass sector. The strip was driven by the constant speed motor; its speed was reduced to one-fiftieth of that of the commutator by means of a worm and gear. There were three brass sectors of different lengths, permitting the giving of stimuli of different durations. In order to measure the intensity of the secondary current a vacuum thermocouple was placed in the circuit and a d'Arsonval galvanometer connected with it.

The intensity of the stimulating current depended in part on the strength of the primary current; but even if the latter remained constant the induced current varied with changes in contact at the surface of the commutator. This factor was not entirely subject to control, but variations in the strength of the stimulating current were kept within 5 per cent.

The frequency of the current depended on the speed of the motor. This showed no measurable variation over short

[2] Professor Harvey B. Lemon and Mr. Barton Hoag, of the Department of Physics, very kindly assisted with the setting up of this apparatus.

periods—three or four weeks—and varied less than 2 per cent. from beginning to end of the experiment.

The duration of the stimulus depended partly on the speed of the motor and partly on variable factors such as the wear of the contacts. It varied considerably in the course of the experiment, but not more than two sigma in any one day. The total range for the first sector was 46 to 54 sigma, for the second, 90 to 97, and for the third, 396 to 416. In the last case the variation was immaterial, for the duration exceeded the subjects' longest reaction times.

Procedure

An extreme degree of relaxation was called for in the present experiment; a degree which our subjects attained only after long practice. The practice period covered about three months, the subjects coming three times a week for an hour and a half each time. The instruction was given by Dr. Jacobson according to a method which he has developed for use in clinical and investigative work (4–8). Its essential feature is that the subject is taught to diminish muscular contraction in a progressive manner. He learns to recognize and to localize the faint sensations present with even a very low degree of contraction, and to relax more and more until the sensations are no longer distinguishable. The chief muscle groups of the body are taken up successively, beginning with the biceps and ending with the finer muscles of facial and vocal expression. At the end of his training the subject has learned to relax all these groups simultaneously.

There are several means by which an observer may apprehend the subject's progress. If he is thoroughly relaxed he will look perfectly limp and his face will be expressionless; there will be a complete absence, during a long period of time, of even the slightest movements—with the exception of course of the respiratory movements, which will be noticeably retarded. If he is not well relaxed the observer will be sure to notice occasional movements, such as twitching of hands or feet, winking of eyelids, eye movements, or twitching of the facial muscles. Furthermore, any considerable degree of relaxation tends to induce sleep.

One person (Subj. *G*) never learned to relax according to the systematic method followed by the others. He did not succeed in distinguishing well the sensations from different muscles, or in relaxing particular muscle groups at will. He described his method by saying, "I just lie down and relax"; and this procedure usually led almost immediately to his going to sleep. However, as there seemed no doubt when one observed him that he actually did succeed in getting fairly well relaxed, it was decided to use him in the experiment. Seven persons, three men and four women, served as subjects. Five of them were graduate students in psychology; two were university graduates with psychological training.

A preliminary series was taken with each subject in order to determine the strength and duration of the stimulus to be used. It seemed best to secure a reaction of considerable extent, in case the response should diminish with habituation. With some individuals this was an easy matter, as the arm movement increased in extent when the current was increased in strength or in duration. With others, the added response took the form of a general stiffening of all the muscles; and in this case the experimenter had to be content with a relatively restricted movement. It was found too that a weak stimulus might call out a marked reaction the first three or four times and little or none after that. In each case the series was continued until it seemed probable that a stimulus had been found which would continue to evoke an adequate response over a sufficiently long period of time; but in spite of this precaution it was found advisable with certain subjects to increase the stimulus, either in strength or in duration, at some point in the course of the final experiment. Such changes do not affect the validity of the results presented, for in all cases conclusions are drawn only from the comparison of groups of reactions taken from a single subject under constant conditions of stimulation. Table 1 shows the duration of the stimulus used for different subjects and gives the data for the factors which determined its intensity. A low reading for the scale of the induction coil indicates a high degree of current strength.

The series constituting the final experiment consisted of approximately 200 reactions for each subject. Half of these were taken under the condition of general relaxation, half with the subject in his ordinary or 'normal' state. The subjects came for an hour a day and in this period ten reactions were taken. The two conditions were employed alternately. In some cases, five reactions under one condition were followed at the same sitting by five taken under the other condition. When the relaxed condition came first on

TABLE I

STRENGTH AND DURATION OF STIMULUS

Subj.	Series	No. of React'ns	Prim. Current (Amperes)	Scale Reading of Inductorium	Duration of Stimulus (Sigma)
A	I*a*	94	.2	10.2	400
	I*b*	100	.2	9.0	400
B	I	200	.35	7.0	50
C	I*a*	74	.2	6.0	50
	I*b*	90	.2	6.0	400
D	I	200	.35	7.0	95
E	I*a*	117	.2	7.0	400
	I*b*	80	.2	6.5	400
F	I*a*	79	.2	6.0	50
	I*b*	100	.2	6.5	400
G	I	184	.35	9.5	50

one day the normal was given first on the next, and so on. In other cases a whole sitting taken under one condition was followed on the next day by a sitting taken under the other. This method obviated the difficulty experienced by some subjects of getting out of the relaxed condition once they had induced it. This was the method followed with Subjects *A*, *E*, and *F*.

The instructions were as follows.

General: "All that you are asked to do in this experiment is to maintain such conditions of relaxation or contraction as may be directed. Several times during the hour you will receive a slight electric shock in your fingers. Please do not prepare to react to this shock in any particular way. If the natural reaction is a movement, let it come freely and spontaneously. Do not try to hasten it, or to inhibit it, or to

influence it in any way. Try not to think about the coming stimulus."

For relaxation: "Please relax as completely as you can; do not stop until you are told to. If the stimulus disturbs you, begin relaxing again as soon as it is over. You will be given plenty of time to get well relaxed before any stimulus is given."

For the normal condition: "Do not relax this time. You are not asked to do anything in particular except to be sure that you are not relaxing. Please keep this in mind, and if necessary move a little occasionally to be sure that you are not relaxing unconsciously. You may talk as much as you like."

The stimuli were given without warning from three to five minutes apart. Care was taken not to give a stimulus while the subject was moving or talking; with the relaxed condition, no shock was given until the subject seemed to be well relaxed. The experimenter observed the subject's reaction, noting what bodily response occurred in addition to the arm movement. At the end of the hour a note was made of any observations the subject had to offer. The subject was questioned as to his success in carrying out instructions and in the case of relaxation was asked whether he had been asleep or not. If there was any doubt on this point, in either subject or experimenter, the subject was questioned further as to the number of stimuli he could remember and on any other points that might throw light on the question.

Results

The results of the experiment are summarized in Tables 2, 3, and 5. Those for Subjects *A*, *B*, *C*, *D*, *E*, and *F* are alike in their general features and may be considered together; those for Subject *G* are radically different and must be reserved for later discussion.

Extent of Movement.—Tables 2 and 3 show that the amount of movement produced was markedly less with relaxation than without. Table 2 gives the number of cases in which movement was reduced to zero; that is to say, in

which the stimulus failed to produce any response large enough to be measured. Table 3 gives the average extent of movement, with these zero cases excluded. It will be

TABLE 2

PERCENTAGE OF CASES WITHOUT MOVEMENT

Subj.	Series	Normal		Relaxed	
		No. of Cases	% without Movement	No. of Cases	% without Movement
A	I*a*	44	0	50	100
	I*b*	50	0	50	92
B	I	100	1	100	36
C	I*a*	37	0	37	19
	I*b*	45	0	45	49
D	I	100	0	100	75
E	I*a*	60	0	57	11
	I*b*	40	0	40	25
F	I*a*	39	0	40	48
	I*b*	50	0	50	66
G	I	92	0	92	28

seen at once from Table 2 that with relaxation all six subjects failed in a considerable proportion of cases to give any measurable reaction, while with the normal condition this

TABLE 3

AVERAGE EXTENT OF MOVEMENT

Subj.	Series	Normal		Relaxed	
		No. of Cases	Centimeters[1]	No. of Cases	Centimeters
A	I*a*	44	5.6	0	—
	I*b*	50	6.1	4	.7
B	I	99	8.1	64	1.2
C	I*a*	37	1.3	30	.9
	I*b*	45	6.1	23	.8
D	I	100	2.0	25	.8
E	I*a*	60	2.7	51	.8
	I*b*	40	1.7	30	.4
F	I*a*	39	.7	19	.4
	I*b*	50	.0	17	.3
G	I	92	8.9	67	—[2]

[1] Centimeters measured on the drum; the actual movement was about twice as great.

[2] Average could not be computed because in many cases the extent of movement exceeded the recording capacity of the apparatus.

occurred only once, with one subject. Similarly, Table 3 shows that the average extent of the movement, when it did occur, was less for all six subjects when under relaxation.

It will be noticed that no measure of variability is included in the tables. The results were highly variable, and such a measure would probably have given no indication that the differences found were significant. Their significance was tested, however, in another way. An average was taken for each condition, for each sitting. In cases where both conditions were given at a sitting the average for the relaxed condition was compared with the normal average for the same sitting; in the other cases, the comparison was between the averages for two successive sittings. Table 4 shows the total number of such comparisons for each subject in each series, and the number of cases where the extent of movement was less for the relaxed than the normal condition. In drawing up these averages zero cases were excluded. When all the relaxed reactions for a given sitting showed zero movement, however, the sitting was included in the table. Inspection of this table leaves no doubt of the significance of the difference in extent of movement resulting from the two conditions.

TABLE 4

NUMBER OF CASES SHOWING MOVEMENT LESS FOR RELAXED THAN FOR NORMAL CONDITION

Subj.	Series	Total No. Cases	Movement Less with Relax'n
A	I*a*	5	5
	I*b*	5	5
B	I	20	20
C	I*a*	8	6
	I*b*	9	9
D	I	21	21
E	I*a*	6	6
	I*b*	4	4
F	I*a*	4	4
	I*b*	5	5
G	I	19	2

Reaction-Time.—In general the reaction-time was longer with relaxation than without. Table 5 shows that for Subjects *A*, *B*, *C*, and *E* the average time is longer with

relaxation than with the normal condition; and the fact that this is a significant difference appears from Table 6. For

TABLE 5

AVERAGE REACTION TIME

Subj.	Series	Normal		Relaxed	
		No. of Cases [1]	Sigma	No. of Cases	Sigma
A	I*a*	34	169	0	—
	I*b*	38	163	4	222
B	I	94	125	61	166
C	I*a*	32	120	25	142
	I*b*	42	130	18	156
D	I	79	188	20	174
E	I*a*	53	117	43	145
	I*b*	29	128	22	150
F	I*a*	37	120	16	138
	I*b*	36	188	17	142
G	I	82	190	53	182

Subjects *D* and *F* however the case is different. Table 5 shows that for Subject *D* the average reaction-time is longer with the normal condition than with relaxation, and for Subject *F* the average, though longer for the relaxed condition

TABLE 6

NUMBER OF CASES SHOWING REACTION-TIME LONGER FOR RELAXED THAN FOR NORMAL CONDITION

Subj.	Series	Total No. Cases	Relaxed Exceeds Normal Time
A	I*a*	0	—
	I*b*	4	4
B	I	18	18
C	I*a*	8	7
	I*b*	8	8
D	I	9	5
E	I*a*	6	6
	I*b*	4	4
F	I*a*	4	4
	I*b*	4	1
G	I	16	5

[1] The number of cases represented is less than in the table for extent of movement (3), partly because there are no reaction-times corresponding to zero movement and partly because, in a few cases, defects in the graphs made it impossible to calculate the time.

in Series I*a*, is longer for the normal in Series I*b*. A closer analysis of the results is necessary to show whether these averages represent consistent tendencies.

In the first series taken, Subject *F* gave results similar to those of other subjects. The average reaction-time was longer with relaxation than without, and the difference was consistent for all of the separate comparisons. In the second series, which differed from the first in that the stimulus was increased in duration and slightly decreased in strength, the average reaction-time was longer for the normal condition. Table 6 shows that this relation holds for three of the four separate comparisons. The frequency distributions shown in Figs. 1 and 2 throw further light on the subject. The

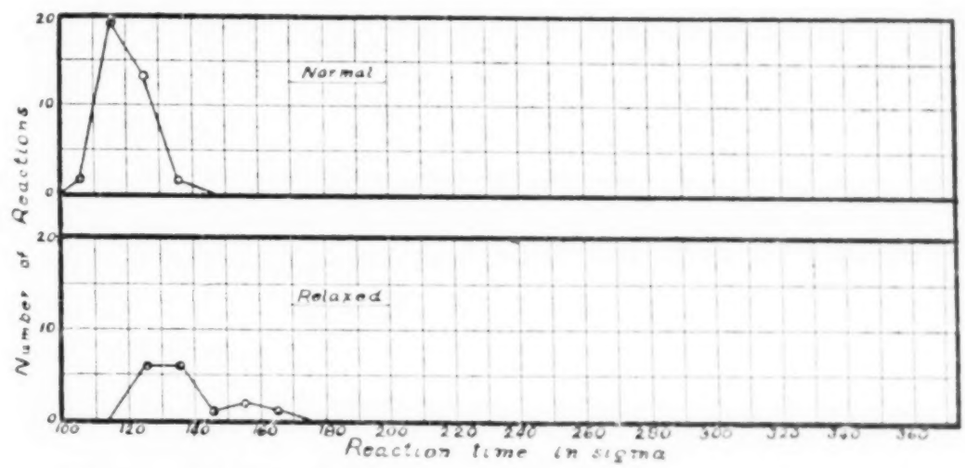

FIG. 1. Frequency distribution of reaction-times, Subject *F*, Series I*a*.

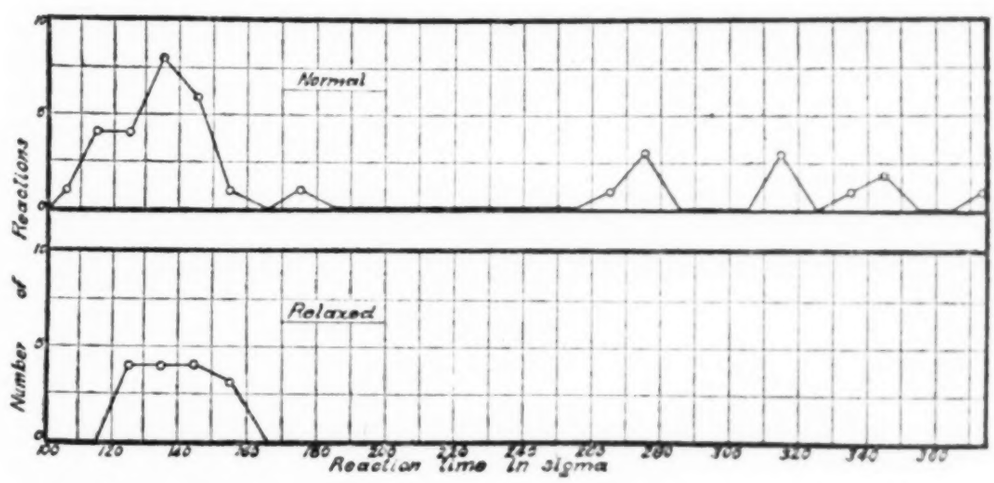

FIG. 2. Frequency distribution of reaction-times, Subject *F*, Series I*b*.

values for the relaxed condition are distributed quite similarly in the two series; but in Series I*b* there occurs with the normal condition a small group of peculiarly long times. Without these, the normal average would be shorter than the relaxed.

An analysis of the results for Subject *D* gave similar results. In each case it was a small group of very long reaction-times, appearing late in the series with the normal condition, which made the average for this condition longer than that for relaxation.

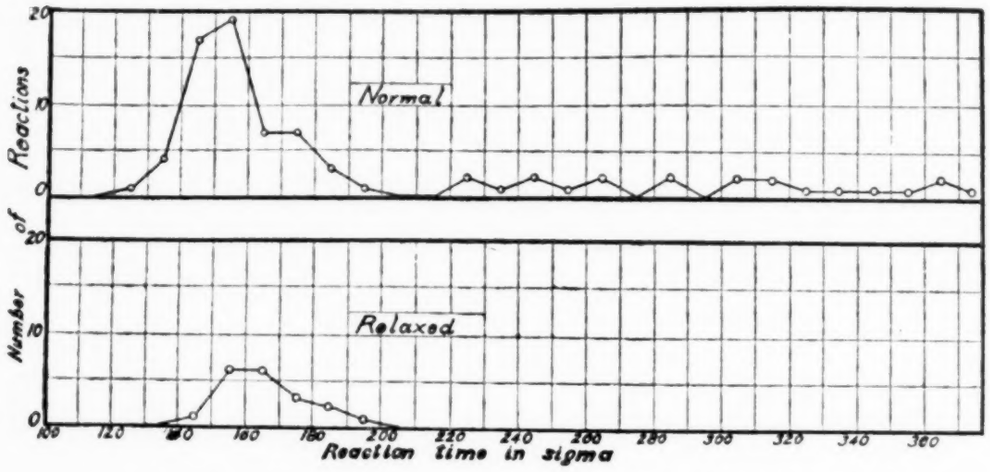

FIG. 3. Frequency distribution of reaction-time, Subject *D*, Series I.

In the effort to explain these peculiarly long reaction-times it was necessary to take into account both the graphic records and the written reports of the subjects. These reports showed certain characteristics to be peculiar to Subjects *D*, *F*, and *C*. It has been mentioned that, in the preliminary series, certain individuals were found from whom no available stimulus could elicit a very extended arm movement. Subjects *D* and *F* throughout, and Subject *C* in Series I*a*, retained this characteristically restricted movement. They also retained a mode of response peculiar to them, a tendency to react with a widespread stiffening of the muscles without extended movement of any part of the body. Observation of this reaction suggested that it represented movements vigorously initiated by numerous muscles but checked almost immediately by the contraction of their antagonists.

Certain other considerations suggested that this tendency might have been related to the lengthened reaction-times under discussion. The study of the graphic records of individual reactions showed a correlation between the length of the reaction-time and the form of the arm movement. For these subjects the typical reaction curve for the normal condition shows two distinct peaks, clearly indicating a double movement. In some cases, however, there is a single peak, and in others the first peak is followed by another so small as to be indistinguishable from a mechanical rebound from the first movement. With the reaction curves classified as double, single, and doubtful, their corresponding reaction-times were listed as normal or lengthened, on the basis already determined from inspection of their frequency distribution. The result is shown in Table 7. No figures are given for Subject *F* in Series I*a*; in this series all the curves were double and there were no lengthened reaction-times.

TABLE 7

RELATION BETWEEN TYPE OF CURVE AND REACTION-TIME

Subject and Series	Type of Curve	Number of Reaction Times		
		Total	Normal	Lengthened
Subj. *D*, Series I	Double	53	52	1
	Single	11	2	9
	Doubtful	15	7	8
Subj. *F*, Series I*b*	Double	24	23	1
	Single	10	1	9
	Doubtful	2	1	1

The close correlation between the long reaction-times and the single form of curve suggests that the lengthened times are to be accounted for by the absence of the first movement represented in the double curve; and this in turn may be due to a contraction of anatagonistic muscles giving, momentarily, complete inhibition of the upward movement. In the case of the double curve this inhibition would seem to have come later and to have given place in turn to the persisting tendency to upward movement. This explanation was partly con-

firmed by a remark of Subject *D*, who said after one particularly delayed reaction, "I felt myself shrinking up and getting stiff before my arm moved."

Subject *C* was the only other to display the tendency to a general muscular stiffening, and—with a very few exceptions in individual reactions—the only one to show double curves. In Series I*b* all but three of his records were of the double type; of the exceptions, two showed reaction-times quite outside the general range. In Series I*a* on the other hand there were few double reactions and no correlation between the type of curve and the length of the reaction-time. It is possible that in this series the only effect of the tendency to stiffening was inhibition of the latter part of the normal reaction movement,—a conclusion suggested by the limited extent of the movement.

If this explanation of the reaction curves is true, it is evident that the real beginning of the response, *i.e.*, the contraction of the muscles which oppose upward movement, occurred earlier than the reaction-times would indicate. In conclusion, it may be said that on the whole, with Subjects *D* and *F* as well as with the others, reaction-times tend to be longer with relaxation than without. The apparent exceptions can probably be explained by certain peculiarities of the reaction under the normal condition, peculiarities which may in some cases obscure the real beginning of the response.

General Bodily Response.—It has been mentioned that the arm movement called forth by the stimulus was usually accompanied by some other bodily response, and the question arises whether a decrease in the extent of arm movement represented a generally diminished reaction. So far as could be determined by gross observation, this was uniformly the case.

Subjective Effect.—A further difference between the results obtained with the two conditions appears in the reports of the subjects. Without being questioned on this point each subject (Subject *G* still excepted) reported the stimulus as less painful, less disagreeable, or apparently weaker, with relaxation than without. Each individual remarked upon

this effect more than once, and they were all questioned about it at several later sittings as well as at the end of the series. There were two occasions, occurring with the same subject, when the effect was not observed; otherwise it was reported as the invariable accompaniment of relaxation.

Effect of Sleep.—During the experiment proper, as well as in the practice period, relaxation tended to induce sleep, and it may be asked whether the supposed effects of the relaxed condition may not have been due to sleep rather than to relaxation as such. In order to answer this question the results were tabulated separately for sleep and waking. It was difficult to determine in every case whether the subject had been asleep or not; but the benefit of the doubt was always given to sleep. If the subject was thought to have been asleep for any part of a sitting, all the reactions of that sitting were put under the sleep heading. The reactions so listed included all those of sittings (1) where the subject said he had been asleep, (2) where he was not sure whether he had slept or not, and (3) where, although he was not aware of having slept, the sitting seemed unusually short to him or he was confused about the number of stimuli received. Of a total of 472 'relaxed' reactions for five subjects, 140 were thus classified as 'sleeping.' Subject *E* did not sleep at any time.

The exclusion of these sleeping reactions did not alter in any respect the conclusions drawn from the original comparison between the normal and the relaxed condition. The differences were unchanged in direction and remained significant. So far as could be judged, however, from the limited number of cases available, the effect of sleep was slightly to exaggerate the differences found with relaxation alone. But this effect was not invariable with all subjects and all criteria. More specifically, it was found that with two subjects sleep increased the percentage of zero reactions, decreased extent of movement, and lengthened reaction-time. With one subject the opposite effect was found. With one, sleep increased zero cases and lengthened reaction-time but also increased extent of movement; and with another it decreased

extent of movement but also decreased zero cases and shortened reaction-time.

The attempt to isolate the effect of sleep was based on the assumption that sleep and relaxation might be independent factors. There remains, however, the possibility that the effect of sleep may itself be due to a heightened degree of relaxation prevailing with that condition.

Subject G.—The results for Subject *G* are in striking contrast to those so far presented. With relaxation almost all of the reactions of this subject fell at one of two extremes: either the stimulus evoked no measurable response or, more frequently, the reaction was far greater in extent than under the normal condition. The average extent of movement for relaxation cannot be given, for the reaction in the majority of cases exceeded the recording capacity of the apparatus. The frequency distributions of Fig. 4, however, show how decidedly the relaxed reactions exceeded the normal. The reaction-times, as one might expect from this fact, are longer for the normal than for the relaxed condition, though the difference is small and its significance not well established. Tabulation of daily averages showed that this relation held for only 11 out of 16 separate comparisons.

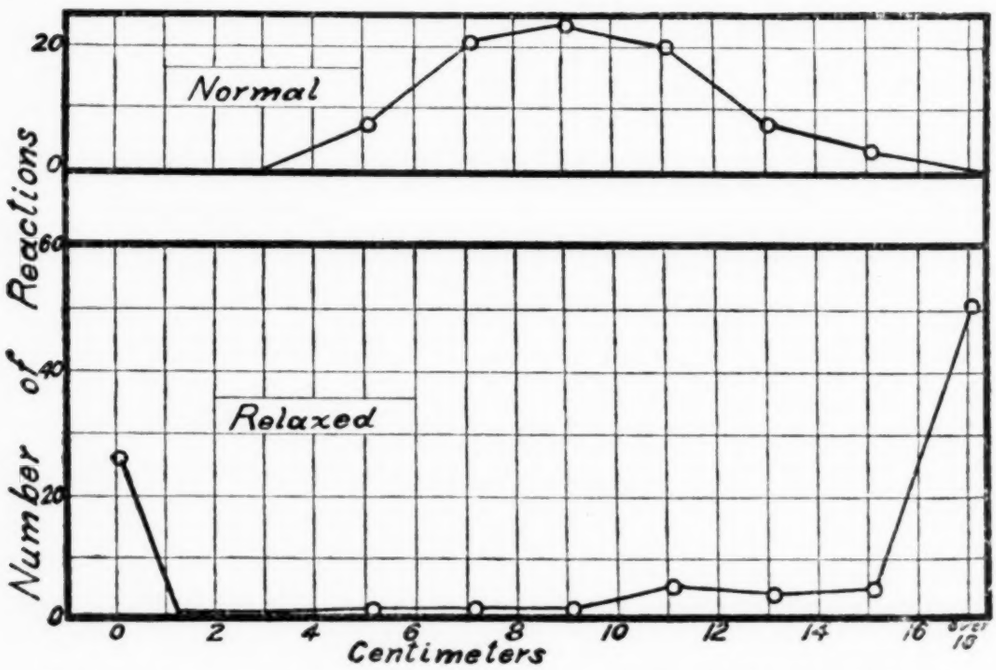

FIG. 4. Frequency distribution of extent of movement, Subject *G*, Series I.

Observation indicated that the extent of the general bodily response was closely correlated with that of the arm movement; the subject's facial expression especially showed that he was much more disturbed by the stimulus when he was relaxed than otherwise.

When this subject tried to relax without sleeping he found it almost impossible; there was in fact only one sitting where he did not sleep. The results of this sitting did not differ from those of the others.

When asked whether the stimulus was more disagreeable with relaxation or without, Subject *G* replied that it was much worse when he was relaxed. He said, however, that when he was relaxed he never felt the stimulus itself. It was his own arm movement, apparently, that awoke him, and it was being disturbed that he found so disagreeable. At the sitting where he did not sleep he reported the stimulus just the same under the two conditions.

Subject *G* showed no sign of the tendency to general stiffening of the muscles observed with some subjects; the arm movement was an extended one even with the normal condition, though the stimulus was much less intense than that used with most subjects. The graphic records showed no peculiarities which could be correlated with the results under discussion.

Summary and Discussion

Reactions to an induction shock were taken under a normal or control condition and under one of general muscular relaxation. In general, the effect of relaxation was to reduce the extent of the movement, increase its reaction-time, and diminish the apparent intensity or unpleasantness of the stimulus. With relaxation there was a considerable proportion of cases where no movement appeared. These conclusions held true whether the subject was asleep or awake under the relaxed condition; the differences were slightly more pronounced when he was asleep.

An exception was found in the case of one subject out of seven. With this individual there were a few cases, with relaxation, where no movement appeared; but if it did occur

it was much more extended and the reaction-time was shorter than with the normal condition.

That motor conditions, both general and local, may affect ensuing movements is of course an accepted fact. One need only mention, in the case of reflexes, the extensive literature of facilitation and inhibition. In the case of voluntary movements the effect of contraction distributed through various parts of the body is recognized in the so-called law of dynamogenesis; while the facilitating effect of preliminary contraction of the reacting muscles has been well brought out in reaction-time experiments, particularly in the work of R. D. Williams (15).

The closest analogy, however, to the conditions of the present experiment is found in the work on sleep. Recent workers in this field (Coriat, 2, Piéron, 12, Kleitmann, 9) have emphasized the muscular relaxation accompanying sleep and the efficacy of such relaxation in producing its onset. Piéron dwells at some length on the connection between sleep and relaxation, although he emphasizes primarily a hypothetical substance called hypnotoxin; Coriat and, more recently, Kleitmann consider relaxation the primary factor. Jacobson (3) found during experiments with an olfactometer that the instruction to his subjects to abandon all effort of attention led to their becoming markedly relaxed and in extreme instances going to sleep. Records were taken of the behavior of the subject, and these were the precursors of the method of relaxation used in the present experiment. Jacobson (4, 5, 6) also reported the use of this method in clinical studies, finding that when relaxation had sufficiently progressed the subject went to sleep. The tendency of our own subjects to go to sleep under the condition of relaxation has already been described.

The parallel extends not only to the conditions of the experiment but also to the results obtained. Along with relaxation there also appears in sleep a general diminution of reflex excitability; this is the conclusion reached by Piéron after an exhaustive review of the literature. Sometimes this reduced excitability has been shown by lengthened reaction

time (Piéron, 12, Tarchanoff, 14), sometimes by absence or reduced extent of movement (Rosenbach, 13, Lombard, 11, Bowditch and Warren, 1, Lee and Kleitmann, 10), and sometimes by a rise in the threshold of excitation (Tarchanoff, Piéron). In contrast, however, to the diminution of excitability which appears with sleep through the greater part of its course, Rosenbach found a temporary stage in which irritability was increased; this might appear just before the subject went to sleep and persist for a few minutes while his sleep was still light. Piéron reported a similar temporary increase just before waking. The similarity between the results found with sleep and those of the present experiment grows even closer when one recalls that in both cases muscular relaxation and lowered reflex irritability are accompanied by reduced sensitivity to sensory stimuli.

A current theory in regard to sleep is that this raising of the threshold for both reflexes and sensation is due to the shutting off of incoming sensory impulses; some of the excitations shut off are those from external stimuli, some from the muscles. These last are reduced by the process of relaxation. This theory applies equally well to the present experiment. In this case external stimuli, as well as afferent impulses from the muscles, were reduced with the condition of relaxation; for the subject relaxed with eyes closed, and the room was quiet except for the monotonous sound of the motor. An interesting question for further experiment is how much of this reduction of muscle contraction is necessary to obtain the decrease of excitability. With sufficiently trained subjects it may be possible to determine the result of relaxing the arm alone, or the rest of the body without the arm.

The most puzzling problem which remains is that presented by the exceptional results obtained with Subject *G*. The fact that this subject was less systematic than the others in his method of relaxing suggests that some difference in the kind or degree of his relaxation may have been responsible for the peculiarities of his reaction. It is further suggested by the observations of Rosenbach and Piéron that there may be an early stage of relaxation where excitability is increased

rather than diminished. This suggestion is borne out by the common clinical observation that a subject must relax to some extent before the knee-jerk can be obtained. It is possible that Subject *G* never passed such an early stage of relaxation. On the other hand, it is strange that, if such a stage exists, exaggerated reactions were not found at some time with other subjects; for their success in relaxing certainly varied from time to time, and in spite of all precautions stimuli were sometimes given when they were not fully relaxed. Further experiment is necessary to determine the validity of this conception.

Another suggestion in regard to Subject *G* came from the fact that he had had long experience with electric shocks in a conditioned reflex experiment, and had acquired a peculiar dread of them. It is possible to conceive that, if the neural mechanism for this response had thus been raised to an unusual degree of excitability, it might retain its readiness to respond after other interfering or inhibitory systems had yielded to the effect of relaxation.

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LIMITS OF LEARNING BY TRIAL AND ERROR

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The terms and the conception of 'trial and error learning' seem to have come from Lloyd Morgan.[1] Previously to the development of this conception it was not understood that the individual is a mechanism whose 'will' is dependent upon the training of an innate organization or structure. Will was usually regarded as a force or faculty by which we could order or direct our acts in advance of learning those acts. Lloyd Morgan's conception was at first applied only to animals, and even today many writers on psychological topics—to say nothing of writers on popular topics—seem to regard rational, voluntary behavior as simply the regulation of activity by certain ideas somehow mysteriously picked up, as it were, from being told how to do a thing or from seeing others do it. This erroneous view is found particularly in literature bearing on ethics; but it also occurs rather extensively in educational and even in psychological literature. Learning is frequently divided into learning 'by ideas' and learning by trial and error. In many instances the former kind of learning is regarded as being free from the random excess movements characterizing the latter kind. That is to say, the early trial-and-error stage of acts, which later are well controlled, is often overlooked.

In certain skilled acts in which errors are not very obvious, learning, or the acquirement of skill, is not infrequently regarded as merely the result of repetition. On this view practice simply involves a repetition of perfectly accurate acts for the result of greater speed in the performance. Here we think of such things as addition by adults who are careful and accurate, as typewriting by one who has good control of

[1] See his *Introduction to comparative psychology*, 1894, Chap. 12.

finger-movements and who uses the visual method so carefully that no wrong keys are hit, or as card-distribution, in which great caution is taken against errors. In such activities, with the proper care, no 'errors' are made, yet speed increases regularly with practice; and the usual formula for improvement is very simple—just repeat the performance many times. Such learning as this, in which improvement seems to be only a gain in speed, and in which all gross acts can be voluntarily controlled, with the proper precautions, is by some writers regarded as qualitatively different from learning by trial and error, which displays many excess movements. Trial-and-error has therefore seemed to present a difficulty to theory that has not appeared in the other kind of learning, which we may, for the present, call rational, this difficulty being *how the errors are eliminated.*

Experiments have made it obvious that learning by trial and error is not accomplished simply by repetition; for this process, even if it were possible before the act is learned, would fix wrong as well as right acts. Careful tabulations of successive acts in their actual order, moreover, have shown that the laws of frequency and recency do not account for the selection that goes on in such learning.[1] The view, held by certain writers, that each particular act is a discrete thing, and that in learning by trial and error new acts are not acquired but that erroneous or irrelevant acts are simply dropped off, leaving finally only those which from the first were appropriate, has of late also received some hard knocks. Experiments on reflexes, like those by Sherrington[2] and by Dodge,[3] as well as numerous studies on more far-reaching coördinations involving attitudes or sets, have shown that in any particular act practically the entire organism is involved in a degree, and that therefore in learning there is an overlapping of impulses, a conflict among different systems of coördination. Thus for any particular consummatory re-

[1] Peterson, Jos., Frequency and recency factors in maze learning by white rats, *J. Animal Behavior*, 1917, 7, 344–364; Learning when frequency and recency factors are negative, *J. Exper. Psychol.*, 1922, 5, 270–300.

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sponse there results a balancing of the different factors involved, until finally the successful expression of one system results in the suppression of others. The question as to how learning goes on has therefore taken on of late so many interesting and new aspects that the whole process of learning is coming to be regarded as a central problem in the field of psychology.

The present experiment, begun about seven years ago, was devised to explore the alleged differences between the acquirement of skill in voluntarily controlled acts on the one hand and learning by trial and error on the other. The learning is of a kind that does not, on superficial consideration, seem to allow of any errors, in the usual sense of this term; all responses are correct, but with practice they constantly increase in rapidity. The subject is given a sheet of paper on which are typewritten, in order, the letters of the alphabet, these letters being numbered in a random order from 11 to 36, as shown in Table I. He is instructed as follows:

> On this sheet you will find the several letters of the alphabet each associated with a certain number, written directly under it. The problem is to see how soon you can learn to give each letter immediately when its number is called. I shall call out these numbers in a chance order and for each number given you are to speak out as quickly as possible the corresponding letter. As soon as you give your response each time another number will be called, so that you will have to keep responding to successive stimuli until a certain series has been completed. The series gone through each day will consist of 156 numbers involving each of the letters of the alphabet six times. You may keep this sheet before your eyes during the experiment, and must not respond in any case until you get the right letter. In case you make a mistake you must correct it at once, before the next number is called. With the apparatus which you see here [the operation of which was shown to S] I shall keep an accurate record of the time of each of your responses, and shall count as errors all responses which are not made within a period of 1.5 or 2 seconds after the stimulus has been given. Do the very best you can and see how soon you can go through the entire list of 156 responses with no errors. You will be told each day before practice the number of errors made in the last practice.

TABLE I

A	B	C	D	E	F	G	H	I	J	K	L	M
33	24	35	17	22	36	18	31	20	21	30	14	25
N	O	P	Q	R	S	T	U	V	W	X	Y	Z
32	12	26	13	27	34	16	29	11	19	28	23	15

In calling out the numbers the experimenter followed chance-order schedules varied so that no one sequence of the stimuli would become fixed. Thus no associations were made except those between the letters and their respective numbers. The time of each response was measured by means of a record made on the revolving kymographic drum just above a scale of $\frac{1}{5}$-sec. units registered simultaneously with the responses by means of a Jacquet chronometer. The experimenter touched a telegraph key when the syllable revealing the stimulus-number in any case was called; as twelve′, elev′en, four′teen, thirty-three′, *etc.* He released it as soon as the subject gave the correct response. This method was preferred to the use of a standard chronoscope because the experimenter was obliged to determine whether the response was right or wrong and to disregard the latter, and it was desirable to have records of rapid serial responses. Throughout the experimental series each number-stimulus was called out immediately after the subject gave the right response to the preceding number. The records were shellacked, and, when they had become dry, the number of responses taking a longer period than two seconds were counted as errors. In a second count, with the more rigid criterion, all responses taking longer than one and one-half seconds were listed as errors. Two parallel records of the same reaction-series were thus obtained. In the case of one subject the experiment was continued till all the reactions of two successive days—a total of 312—gave no errors at all even by the $1\frac{1}{2}$-seconds' criterion.

Table II. summarizes the data and Fig. 1 gives the learning

TABLE II

ERRORS MADE (SUBJ. *K*) IN THE FIRST (*a*) AND IN THE SECOND (*b*) HALF OF THE PRACTICE PERIODS ON SUCCESSIVE DAYS

	Days of Practice																					
	1		2		3		4		5		6		7		8		9		10		11	
	a	*b*	*a*	*b*	*a*	*b*	*a*	*b*	*a*	*b*	*a*	*b*	*a*	*b*	*a*	*b*	*a*	*b*	*a*	*b*	*a*	*b*
1.5-sec. Criterion.....	73	56	61	47	55	30	30	29	26	24	17	16	10	8	17	8	7	3	7	3	0	0
2-sec. Criterion.....	63	52	45	38	28	23	24	14	15	14	7	8	4	4	5	1	0	0	0	0	0	0

curves for a thirteen-year-old high-school boy. The errors made in the first and in the last half of each day's practice are given separately, to display possible effects of warming up and of fatigue. Each day's practice began just after the subject was dismissed from school in the early afternoon. A study of the curves (Fig. 1) reveals a greater irregularity

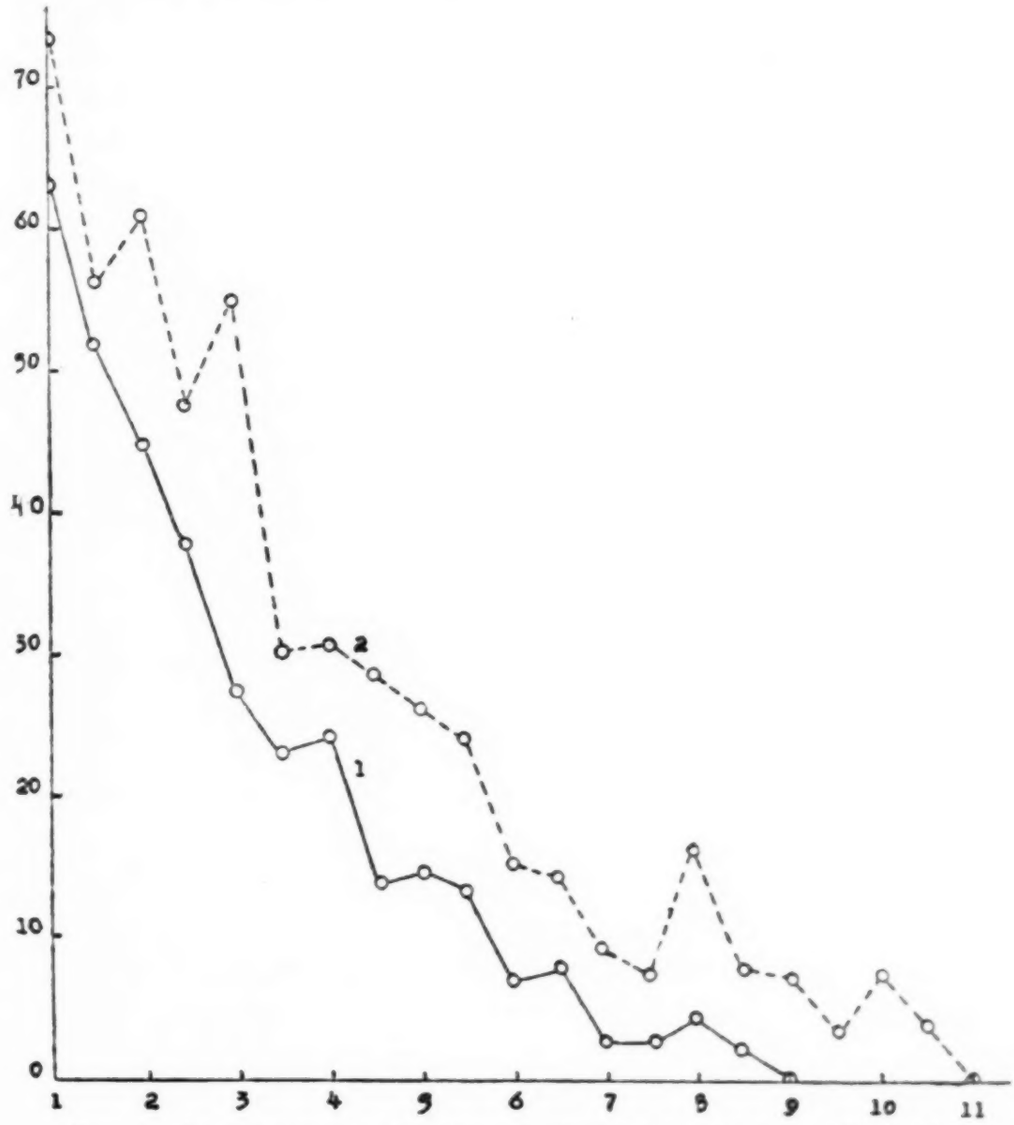


FIG. 1. Learning curves of Subject *K*, plotted from the data of Table II.
Curve 1: errors by the two-seconds' criterion.
Curve 2: errors by the one-and-one-half-seconds' criterion.

in Curve 2, obtained by the more rigid criterion of errors, than in Curve 1, showing that the finer the analysis the greater the irregularity in the responses. That is to say, in

those cases where errors are not made evident by the coarser methods of analysis, finer analysis shows that the learning still has the characteristics of the trial-and-error process of adjustment. In both curves, however, the irregularities are largely concealed by the summing of errors in groups of 78 responses. The individual kymographic records must be examined for details of fluctuation. These were unfortunately not marked in a manner permitting us to identify the individual responses to the several letters, so that, as a consequence, we cannot determine the variabilities in response to any given letter in successive presentations, as would be desirable in a thorough study.

As the experimenter was seated just before the subject, carefully watching his behavior so as to record as accurately as possible the moment when the correct letter was given, an unusual opportunity was afforded to observe the nature of those responses which were more or less delayed. The subject at first kept his eyes on the sheet. When a number within the clear range of his vision was called the response was direct and quick; but for other numbers the eyes made random, exploratory movements here and there, often recurring several times to a given part of the sheet. As the localization of certain numbers became better established by the subject, these ocular movements showed less variability and they were more predictable; and they finally disappeared and became unnecessary as the association of the numbers with their respective letters reached an approximately automatic stage. Hereupon the inhibitions and checks, which the instructions to get the 'right letter' had given to the responses, practically disappeared, and the stimulus directly and automatically brought about the proper reaction of the vocal mechanism. The subject remarked on several occasions that he experienced a feeling of released tension and responsibility at this final stage of the learning. The release of these inhibitions, which were precautionary against the giving of wrong responses, was, however, very gradual, and complete automaticity could be acquired only by long practice through successive days.

This phenomenon of gradual release of inhibition is just what we see also in such trial-and-error processes as the learning of ball-tossing, in which the subject is at first, and for a long period, in a marked tenseness betraying serious conflicts among the several reaction-systems, a tenseness which makes it impossible for him to carry on a conversation or easy mental calculations while tossing the balls, but which gradually disappears until, at a final stage, he can toss the balls while answering difficult questions or calculating. In the present experiment conversations after each practice-period yielded evidence confirmatory of the observations by the experimenter. The subject reported conflicting tendencies to look hastily about in different directions on the sheet, with impulses to go back again and again to certain parts when the required number did not readily come into view, and on several occasions he mentioned the persistence of tendencies to certain eye-movements after the association in question had become so well established that seeing the number and letter was unnecessary. At this stage, however, the movements had settled down to fairly definite responses each with its pull in a given direction. Gradually these gave way to immediate and direct vocal reactions which were stripped of all such tendencies.

In connection with experiments on rational learning, similar conflicting impulses and inhibitions of one action-system by others have been noted.[1] In this case A, B, C, D, E, F, G, H, I and J are to be associated, respectively, with the numbers 9, 6, 2, 10, 8, 1, 5, 4, 7 and 3. The subject has nothing before him and does not know which of the ten numbers belongs to any one letter. All this he is to determine by his own trial guesses, until he remembers the several associations. In the early stages of this test the subject is likely to be so taken up with some detailed aspect of the complex situation presented to him that he overlooks the most obvious and well-known relations. He may, for instance, remember perfectly well that 9 is the number for A, the first letter called out, and therefore avoid guessing 9 for

[1] Peterson, Jos., Experiments in rational learning, *Psychol. Rev.*, 1918, *25*, 443-467.

other letters; and yet when he comes to determine the number for, say, F, which he has had difficulty in establishing, he seems to get into so tense a state as completely to release the inhibition on 9 for other letters than A, and so gives 9, among other random guesses, for F. When asked afterwards why he did this irrational thing, when his record shows that for two or more repetitions he had rightly given 9 for A each time, he replies: "I knew A was 9, but didn't think of it at the time." When in such a state of tension and inner conflict the subject proceeds very slowly, sometimes beginning to give a letter, as 'sev' for seven, and then retracting it; sometimes speaking it out completely, then immediately saying "no!" and guessing another number. Not infrequently after guessing one or two wrong numbers in a hesitating manner, he suddenly speaks out the correct one with certainty. Occasionally the confusion becomes so great and the range of orientation so limited that the same erroneous number is repeated several times among other numbers, as 5, 3, 6, 5, 4, 5, 1 (the correct number). This repetition of a number found to be incorrect is what I have called *perseverative response*, after Hamilton, who found it characteristic of certain animals and individuals of lower intelligence. It occurs in human subjects when they are in great confusion and inner tension. As the Rational Learning test nears completion there is a very noticeable release of tension, the speed of reaction becomes accelerated, and the hesitating, retracting responses change to reactions in which the numbers flow out, as it were, evenly and uniformly, and a look of certainty comes over the subject's face as the inhibitions disappear. The experimenter readily perceives whether a correct repetition of the numbers through an entire series from A to J is accidental or due to a real mastery of the problem. If there is still hesitancy and tension the subject usually makes some error in the next repetition. Two repetitions without error are required. One subject had trouble with the letter E, in the Rational Learning test. Its number is 8. He finally overcame the difficulty sufficiently to avoid error at this point, but in his anxiety about retaining

the connection E–8 he neglected other relations previously learned and gave 8 instead of 10 for D, preceding E. He had for some time been giving 10 correctly for D. This was a case of being so well set for the response 8 at the difficult point that the reaction took place too soon. This sort of behavior is often noticed, and clearly shows overlappings of impulses and conflicts among different attitudes.

It has been shown that similar tensions also appear in the learning by animals as low as the white rat. Some blind-alleys are frequently eliminated by a piece-meal process. At first the animal runs to the end of the blind-alley; but soon it begins to turn a little short of the end. With successive entrances in different trials the degree of penetration becomes less and less, speaking generally, until, at a late stage, there is hesitancy at the approach of the blind-alley in question and a peculiar and rapid vibration of the head takes place. If, at this stage of inhibition, the impulse to enter breaks through, the animal may run with a blow against the end of the blind-alley and may emerge so much disoriented that it enters several more distant blind-alleys which had, for some time, been successfully eliminated under usual conditions. Thus by a single break the whole system of balanced acts becomes in a measure disturbed.[1]

We seem to need more detailed analyses of the different 'kinds of learning,' and more attention to the conflict of impulses in the earlier stages of learning and to the manner of their disappearance as the adjustment becomes more and more definitely established. Except to the most superficial observation, 'learning' is not a detached part of psychology that can be divided off into certain discrete types. In all learning we shall probably find the sort of random, trial-and-error adjustment that is so characteristic of the acquirement of new sensory-motor controls, together with inhibitions and balancings of conflicting tendencies to response. The higher forms of learning-adjustments, involving ideational activity, differ from the usual learning by trial and error in that in such forms the conflicts are among systems of acts already

[1] See *Behavior Monog.*, 1917, vol. 3, no. 15, 28*ff*.

coördinated within themselves for fairly remote, far-reaching adjustments. At any rate, the present study, which is only at the initial stage, seems to indicate that with finer and finer criteria of errors applied to rational and higher forms of learning, random processes similar in essentials to the usual trial-and-error type will be found. The question of how the maladjusted, excess acts become eliminated seems to be basic to all learning, and fundamental to an adequate understanding of all behavior for which the mechanism is not fully determined by innate structures. Instead of being a process of merely combining a number of elementary unit reaction-systems by association, or of 'conditioning,' to use a term now in favor, learning seems to be fundamentally a smoothing out of conflicts among incompatible impulses aroused by a complexity of external circumstances or stimuli, so that the organism can act in a somewhat unitary or consistent manner toward them. In certain cases these conflicting impulses may, of course, be somewhat local and insignificant, as when conditioned reflexes are established; but it is well known that even such processes do not usually work with great regularity, as if detached from all inhibitions by the general attitudes of the individual. It is becoming clear from recent work in many laboratories [1] that biological research is putting a strong emphasis on the unity of the organism and the interaction of its various part-processes. The results of these investigations are in agreement with those which have been pointed out in the foregoing pages, indicating inner tensions which are not overcome by mere frequency and recency factors in association.[2] Factors of inhibition and facilitation seem to play important rôles in effecting the adjustments, however this is accomplished.

As to the value of proper distribution of trials in learning we may consider the learning rates in the present experiment of eight subjects—three college men and one woman, and four high school boys—who were severally taken through

[1] See, for example, Child, C. M., *Physiological foundations of behavior*, 1924; and Herrick, C. J., *Neurological foundations of animal behavior*, 1924.

[2] See also Peterson, Jos., Learning when frequency and recency factors are negative, *J. Exper. Psychol.*, 1922, 5, 270–300.

nine trials of 156 responses each, in one sitting. Five of these subjects went through ten trials, and two through eleven. In Table III. the results are given in the form of median errors per trial, and of the average deviation from the median. It is obvious that the error-elimination was

TABLE III

NUMBER OF ERRORS IN SUCCESSIVE SERIES (156 REACTIONS IN EACH SERIES)

	Series of Repetitions										
	1	2	3	4	5	6	7	8	9	10	11
	(Eight subjects, one sitting each)										
Errors by 2.0-sec. criterion	101	79	64	61	46	35	27	19	13	11	10
Average deviation	8.9	11.0	16.5	17.5	20.0	15.9	13.3	10.3	8.4	7.2	2.0
Errors by 1.5-sec. criterion	135	110	101	84	71	68	53	40	30	31	29
Average deviation	8.4	11.4	18.4	18.8	21.4	20.6	23.0	17.8	13.1	15.0	0
	(Subject *K*, 156 reactions daily for 11 days)										
Errors by 2.0-sec. criterion	115	83	51	38	29	15	8	6	0	0	0
Errors by 1.5-sec. criterion	129	108	85	59	50	33	18	25	10	10	0

much more rapid in the case of the eleven daily practices than in that of the long practice all at one sitting, although there are of course too few subjects to make the results more than tentative. On the view of balancing factors in learning by the interaction of different part-processes, it would seem reasonable to suppose that with equal practice the longer period of adjustment would be superior to the shorter time of learning all in a single sitting, because in the former case more time would be allowed for the settlement of the conflicts and for the working out among the several impulses of some more or less consistent or organically complete reaction.

THE BRADYSCOPE; AN APPARATUS FOR THE AUTOMATIC PRESENTATION OF VISUAL STIMULI AT A CONSTANT SLOW RATE

BY ERWIN A. ESPER

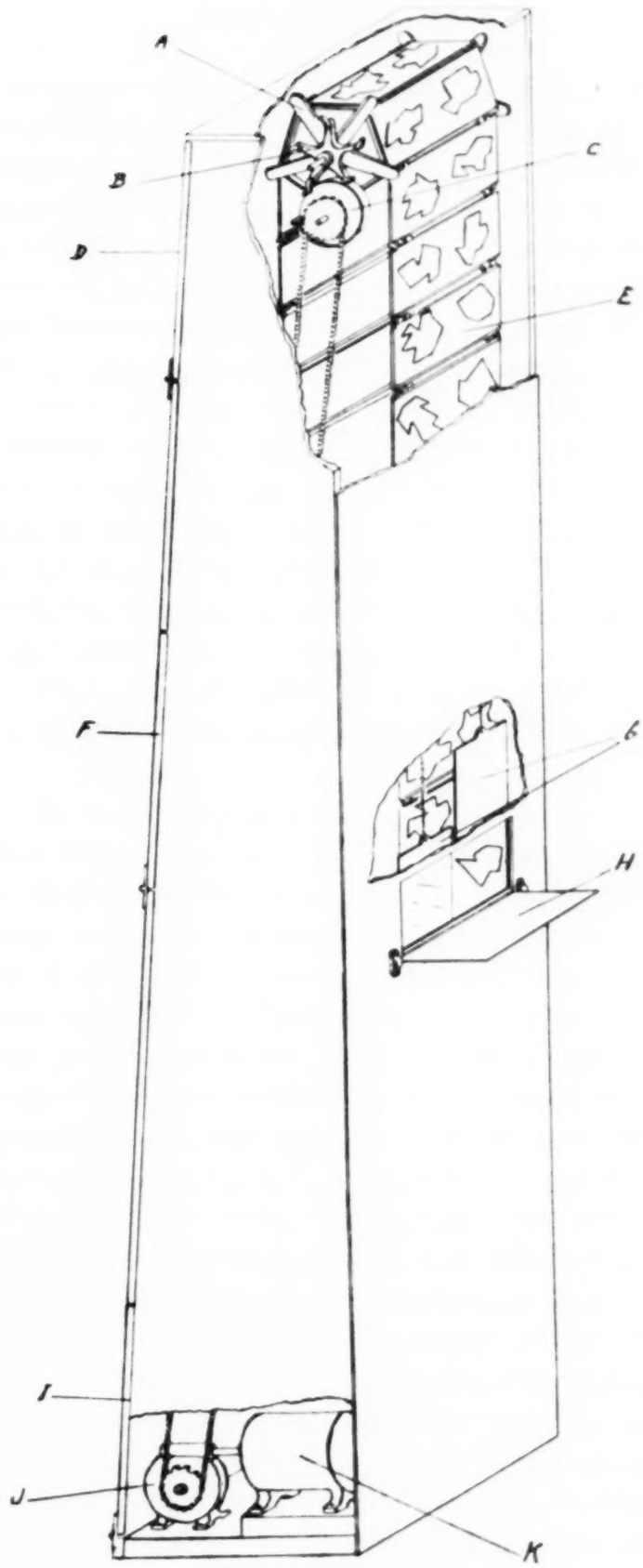
University of Illinois

In a previous publication [1] the writer has described an apparatus for the automatic presentation of visual material exposed serially and at a constant slow rate. This apparatus consisted of a cylindrical drum bearing an endless paper belt on which were pasted the figures to be used. A star-wheel mechanism turned the drum one-fifth of a revolution every 2.7 sec., and also closed a shutter over the exposure-window while the drum was in motion. In the course of six months and more of constant use two main difficulties appeared. First, the paper belt was very easily torn; and secondly, for every change in the order of presentation of the figures a new paper belt had to be prepared and placed on the drum.

In preparing a new series of experiments which require the same type of visual presentation, the writer has devised an apparatus which avoids these difficulties and makes possible quick and easy interchangeability of all the figures of a series.

The apparatus consists of a pentagonal spider drum *A* by means of which an endless chain of metal card-holders *E* is moved. The spiders are aluminum castings mounted on a steel axle. The flanges on the spiders which carry the card-holders are 14.1 cm. in length and 1.6 cm. in width. An aluminum star-wheel *B* is mounted on the same axle as the drum; two prongs of this star-wheel ride on the circumference of the internal (driven) piece of the cone clutch *C*, also of

[1] A technique for the experimental investigation of associative interference in artificial linguistic material. *Language Monog. Linguistic Soc. of Amer.*, 1925, no. 1.



THE BRADYSCOPE

Reduced to $\frac{1}{16}$ actual size. *A*, spider drum; *B*, star-wheel; *C*, sliding external piece of cone clutch; *D*, *F*, *I*, hinged doors; *E*, chain of card-holders; *G*, steel drop-doors; *H*, aluminum shutter; *J*, speed-reducer; *K*, motor.

aluminum. The internal cone piece is 12.4 cm. in diameter. The face on which the prongs ride is notched out to admit a prong of the star-wheel. As the internal cone piece revolves clockwise (as viewed in the figure), a steel roller at the rear edge of the notch engages the finger on the rear riding prong; this prong moves down into the notch, and the drum makes one-fifth of a revolution. On the inner side of the internal cone piece is mounted a cam which operates, by means of a lever and connecting rod, the aluminum shutter *H*. The sliding external cone piece is held out of contact with the internal piece by means of a spring; it is thrown into contact by means of a bell crank, which is operated by a wire cable running down through pulleys to the base of the apparatus, where it passes through a hole to a lever attached on the outside of the case. The latter lever greatly reduces the amount of pull necessary to throw the clutch in gear; it is operated by a cord passing through pulleys to the operator's table.

The power unit is a 1/6 h.p. 110 a.c. motor of 1140 r.p.m. The shaft of the motor *K* is coupled to the shaft of the worm-gear speed-reducer *J*, the speed being thus reduced to approximately 30 r.p.m. The sprockets on the speed-reducer and on the external cone piece of the clutch are related as 2 to 3, so that the upper sprocket and external cone piece make one revolution in three seconds. When the internal cone piece is in gear, the star-wheel and drum are in motion one second and at rest two seconds during each of these 3-sec. intervals. The shutter *H* is closed while the drum is in motion and open while it is at rest. The motor and speed-reducer are enclosed in a felt-padded box; the sound produced by them is thus so reduced as not to interfere with auditory stimuli or verbal responses. The clutch, drum, and card-holder mechanisms are practically noiseless.

The card-holders are 26 x 13.6 cm. They are constructed of two horizontal strips of 16-gauge tin, bent into U-shape to afford slots for the cards, and two vertical strips of 3/64 in. galvanized iron, whose ends are bent into hinges to receive 3/32 in. steel axles. These axles bear hooks by which wire loops secure them in the hinges.

The window before which the shutter *H* is placed is 26 x 13 cm. The chain of card-holders passes behind this window through vertical hardwood guides. Either half of the window may be closed by means of the 1/32 in. steel drop-doors *G*, contained in metal guides, which are operated by means of cords passing through pulleys.

The wooden case is 2.6 m. in height and 45 cm. in width. The front is vertical, but the rear slopes from a depth of 46 cm. at the base to 31 cm. at the top. The sides and back are constructed of 7/8 in. pine: the front of 3/8 in. laminated panel stock. Hinged doors at *D*, *F*, and *I* afford access to the mechanism. The height of the case as given permits of a chain of as many as 32 card-holders; if only one half of the window is used, the apparatus therefore has a capacity of 64 exposures of a 13 x 13 cm. area.

The mode of operation is as follows. The cards on which the figures to be used are mounted are placed in the slots of the card-holders, access for this purpose being through door *F*. The operator, seated at his table, controls the apparatus by means of a switch which starts and stops the motor, and three cords, of which one operates the clutch and the others the two drop-doors *G*. If two vertical series of figures have been placed on the cards (as shown in the illustration), one of the drop-doors (*e.g.*, the left) is raised and the other lowered. The motor is then started; a slight pull on the control-cord throws the clutch in gear and sets the drum and shutter mechanism in motion. The series on the left having been completed, the left door may be dropped and the right raised. The drum and shutter mechanism may be stopped at any instant by releasing the clutch.

The writer has coined the name *bradyscope* as being more appropriate for this apparatus, in view of the slow ate of presentation, than the term *tachistoscope* which was applied to the earlier forms of the apparatus. The rate and manner of presentation are well adapted to any experiments in which the subject's responses to visual objects of relatively large area must be recorded in writing, either by the subject himself or by the experimenter.[2]

[2] The Robert D. Simpson Company of Columbus, Ohio, is prepared to duplicate the apparatus.

TWO DEVICES FOR AIDING CALCULATION

BY HERBERT A. TOOPS

Ohio State University

I. A Standard Deviation Device

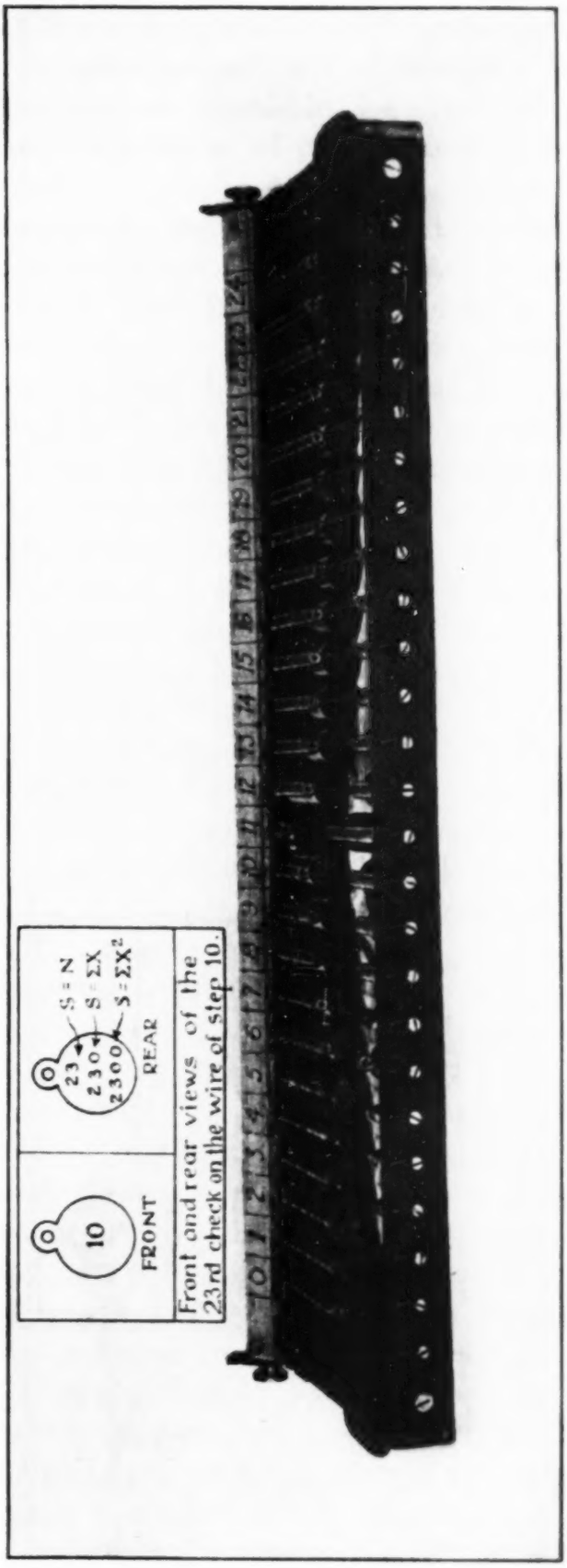
The device shown in the accompanying figure (*A*) has been found very useful in obtaining the frequencies of frequency distributions and in obtaining averages and standard deviations.

Twenty-five piano wires each 7 inches long are stretched between two parallel steel L-bars. The successive wires are 1 inch apart. On each of these wires are hung 50 brass checks, 7/8 inch in diameter, similar to the brass checks attached to door keys in hotels. Each check takes the place of a tally mark in obtaining a frequency distribution. On the front faces the 50 checks of the 0-wire are labeled with a large 0; similarly the 50 checks of the no. 1 wire are all marked 1 on their front faces, and so on.

If the scores of the distribution all lie between 0 and 24 one may pay attention only to the numbers on the faces of the brass checks when making the distribution.

In case the inclusive range of the variable is more than 25, that is, outside the range of 0 to 24, it is necessary to revolve the wooden bar at the top until the fourth face, which is covered with a slate preparation, is visible, whereupon the desired classes may be written thereon in chalk, the checks being now distributed by reference to the classes just written.

The checks being all at the rear of the several wires, one merely pulls forward on the wire a brass check whenever one wishes to make a tally mark corresponding to a frequency of the distribution. Since the separate checks are separated from each other on the wire by a small washer, about 1/32 inch thick, it is very easy to introduce the fingernail behind


10
FRONT
23
230
2300
S = N
S = ΣX
S = ΣX²
REAR
Front and rear views of the 23rd check on the wire of step 10.

the check and slide it forward along the wire. A completed distribution is shown in the forward side of the picture. Part of the distribution is hidden in the picture by the front frame of the device. It will be noted that class-11 has the modal frequency.

The distribution having thus been made, one turns the device around in order to observe the back face of the checks last plotted on each of the several wires of the distribution and now plainly visible from the rear side of the machine. The back face of the twenty-third check of the 10-column, or wire, is shown in the inset diagram of the figure. The top one of the three figures of the check indicates the number of the check; that is, 23 indicates that there are 23 checks in this column in the distribution, thus saving us the necessity of counting them as we should have to do for tally marks. On the next check in the 10-column the top entry would be 24; then on the following one, 25 and so on. That is, we may now copy the frequency distribution by merely copying the top figures showing on all the successive checks respectively. If we are not interested in the distribution itself we may add the top figure on all successive ones of the 25 checks showing and thus obtain N, the total number of cases.

The second figure showing on any check is the step times the frequency, that is, the ΣX component. By adding the second figure showing on all the successive 25 checks we obtain ΣX. In the inset the figure 230 means step-10 times the frequency 23 = 230. *This operation therefore saves the operation of multiplying steps by frequencies*, needed both in the computation of the average and the standard deviation.

The third figure showing on any one check is the square of the step multiplied by its frequency; that is, the ΣX^2 component. Thus on the inset, 2300 is the product of the square of step-10 times the frequency 23. The sum of all such entries on the 25 checks showing gives us ΣX^2. Thus this step saves the operation of multiplying the square of the step by the frequency. The peculiar advantage of the machine is that the counting of frequencies in the successive steps and the extensions of the steps and their squares by

the frequencies have been done once for all by the check-maker, the proper sums and extensions being permanently impressed into the brass checks.

We now have the value of N, ΣX, and ΣX^2. We may substitute these in the usual formulæ for the mean and standard deviation:

$$M_X = Fo_X + \frac{\Sigma X \cdot I_X}{N}, \tag{1}$$

$$\sigma_X = I_X \sqrt{\frac{N\Sigma X^2 - (\Sigma X)^2}{N^2}}, \tag{2}$$

in which I_X is the class interval; Fo_X is the face-value of the zero-step.

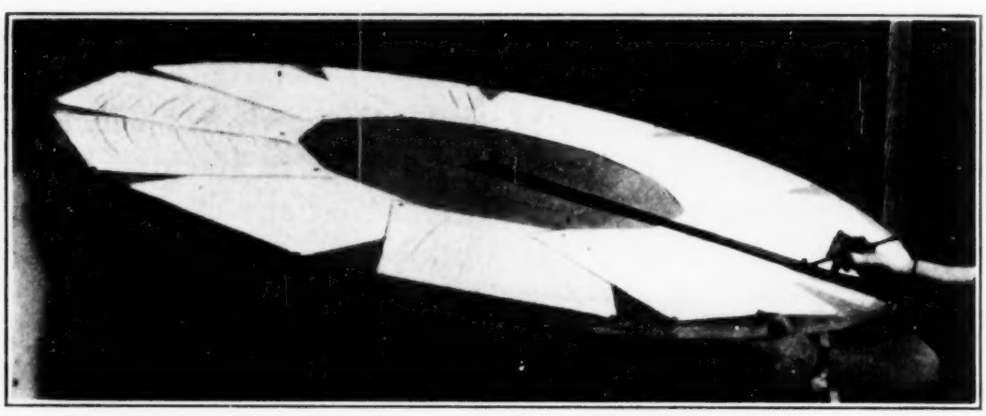
The machine contains 1250 checks no two of which are identical. This results in a fairly expensive machine; but it simplifies the work to such an extent that any clerical worker can satisfactorily obtain distributions, averages and standard deviations.

Face 1 of the bar at the rear of the machine is shown in the picture. On face 2 of the bar, found by revolving the bar in its slide sockets, are found the numbers 25 to 49 inclusive; and on face 3 the numbers 50 to 74 inclusive. The first three faces have been found very useful in tabulating the errors made on individual examination questions in tests given in the objective type, where there are no more than 75 items. This range will include the majority of mid-term and final examinations. By tallying the failures, item by item, one may thus readily determine the degree of difficulty of the several items of mid-term examinations and save up those difficult items, where the difficulty is intrinsic and not due to the wording of the item, for use as final examination questions. Such final questions will yield an excellent dispersion of the students and so improve the academic marks by making the number of 'border line cases' small.

After completion of one distribution the machine is 'set back to zero' by tilting upward the front until the checks rush down hill by gravity to the rear side of the machine.

II. An Intercorrelation Device

A method whereby intercorrelations can profitably be computed by reducing the plotting paper to polar form has been published.[1] By increasing the curvature of the ordinates in this form it is possible to make the center of a number of concentric charts lie within 27 inches of the bottom of the charts; consequently the charts may be arranged in a circle on the top of a rotating table 54 inches in diameter. This allows the worker to sit comfortably on a chair and to have successive charts rotate around to his position.



The figure (*B*) shows 11 charts thumb-tacked in a circle about the top of a soft pine rotating table. The table[2] is adjustable for height and for angle-tilt to suit the worker's height and convenience.

A movable bar, pivoted at the center of the table, contains a slide which may be set at any *Y*-score, 0 to 17 inclusive, of person *A* of test 1. The successive tally-marks of *A* on the correlation charts r_{12}, r_{13}, r_{14}, $\cdots$, r_{1n}, will always occur in the same horizontal row.

Let us assume the following transmuted scores[3] for Subject *A*.

[1] Toops, H. A., Solving intercorrelations by polar coördinates. This Journal, 1922, 5, 60–75.

[2] Such a table may be readily constructed from a discarded drawing stand at a cost of about $10.00 for table and work. The charts used in this method may be obtained from the author.

[3] Toops, H. A., Computing intercorrelations of tests on the adding machine. *J. Appl. Psychol.*, 1922, 6, 172–184.

Test:—1	2	3	4	5	6	7 . . .
Score:—6	8	9	4	10	2	8 . . .

The reader calls out to the tally-marker, 'Set slide on 6.' The tally-marker sets the slide at 6; all tally-marks on the successive correlation charts will now appear opposite the slide in the 6th row. The reader now calls three scores, '8, 9, 4.' The tally-marker moves the table and brings the r_{12} chart with Column-8 showing just to the left of the bar and puts a tally-mark opposite the slide; then brings the 9-column of the r_{13} chart to the bar and enters a tally opposite the slide; and similarly the 4-column of the r_{14} chart. After each tally he calls out his entry to the reader for verification and correction in case of error. As soon as he has called out the series '8, 9, 4,' the reader gives the new series '10, 2, 8,' whereupon the procedure is repeated. With practice the table may be stopped almost exactly at the right column, since the table is light and has little inertia. If not stopped at exactly the right place, the bar is movable and is quickly brought to the right place by the right hand which is used to prevent turning of the bar as the left hand rotates the table.

Practice rapidly improves one's ability with the device. The table below gives the number of minutes required by two persons [4] to plot 10 correlation coefficients of 364 cases each; *i.e.*, 3640 tally-marks, with the increase in practice observed up to 200 coefficients.

Correlation Numbers	Min. Req'd to Plot 10 Correlations	Tally-marks Plotted per Minute	Correlation Numbers	Min. Req'd to Plot 10 Correlations	Tally-marks Plotted per Minute
1–10	250	14.6	101–110	210	17.3
11–20	230	15.8	111–120	210	17.3
21–30	220	16.5	121–130	200	18.2
31–40	210	17.3	131–140	200	18.2
41–50	200	18.2	141–150	190	19.2
51–60	210	17.3	151–160	180	20.2
61–70	210	17.3	161–170	190	19.2
71–80	220	16.5	171–180	190	19.2
81–90	220	16.5	181–190	180	20.2
91–100	210	17.3	191–200	160	22.8

[4] Allen, W. S., A Study in Latin Prognosis. *Teachers Coll. Contrib. to Educ.*, no. 135, 1923, 10–11. The writer wishes to express his thanks to Mr. Allen for the collection of the data here presented.

It will be noted that the limit of improvement has not yet been reached but that the efficiency after having plotted 200 correlation coefficients, in about 70 hours, was 22.8 tally-marks plotted per minute. The initial efficiency was 14.6 tally-marks per minute, the time including the time required to align the papers and plot the correlations, two people working together in the process. This means the time of one person for roughly 140 hours was required to plot the 200 correlations of 364 cases each. Inasmuch as the work can be done by relatively unskilled clerical workers the saving is obvious. It is to be hoped that other workers will publish figures showing the efficiency of other methods, subdivided according to time required for plotting, for making extensions, and for making all final computations.

A NON-ELECTRICAL ROTATION-TABLE FOR LABORATORY ANIMALS [1]

BY F. S. FEARING

Department of Psychology

AND

F. W. WEYMOUTH

Department of Physiology, Stanford University

The interest in problems connected with the responses to stimulation of the non-acoustic labyrinth has turned the attention of investigators in this field to the development of rotation apparatus. The apparatus described in the present paper was developed in connection with a program of research on labyrinthine functions in the pigeon being carried out in the Department of Anatomy at Stanford University. These investigations called for a rotation-table which would be constant as to (*a*) the rate of rotation and the number of revolutions, and (*b*) the rate of acceleration and retardation.

In the investigations in which this table was used a non-electrical form of motive power was deemed advantageous. Maxwell [2] has described such a table, certain features of which we have retained. Our most important modification concerns the method of imparting an initial impulse to the table.

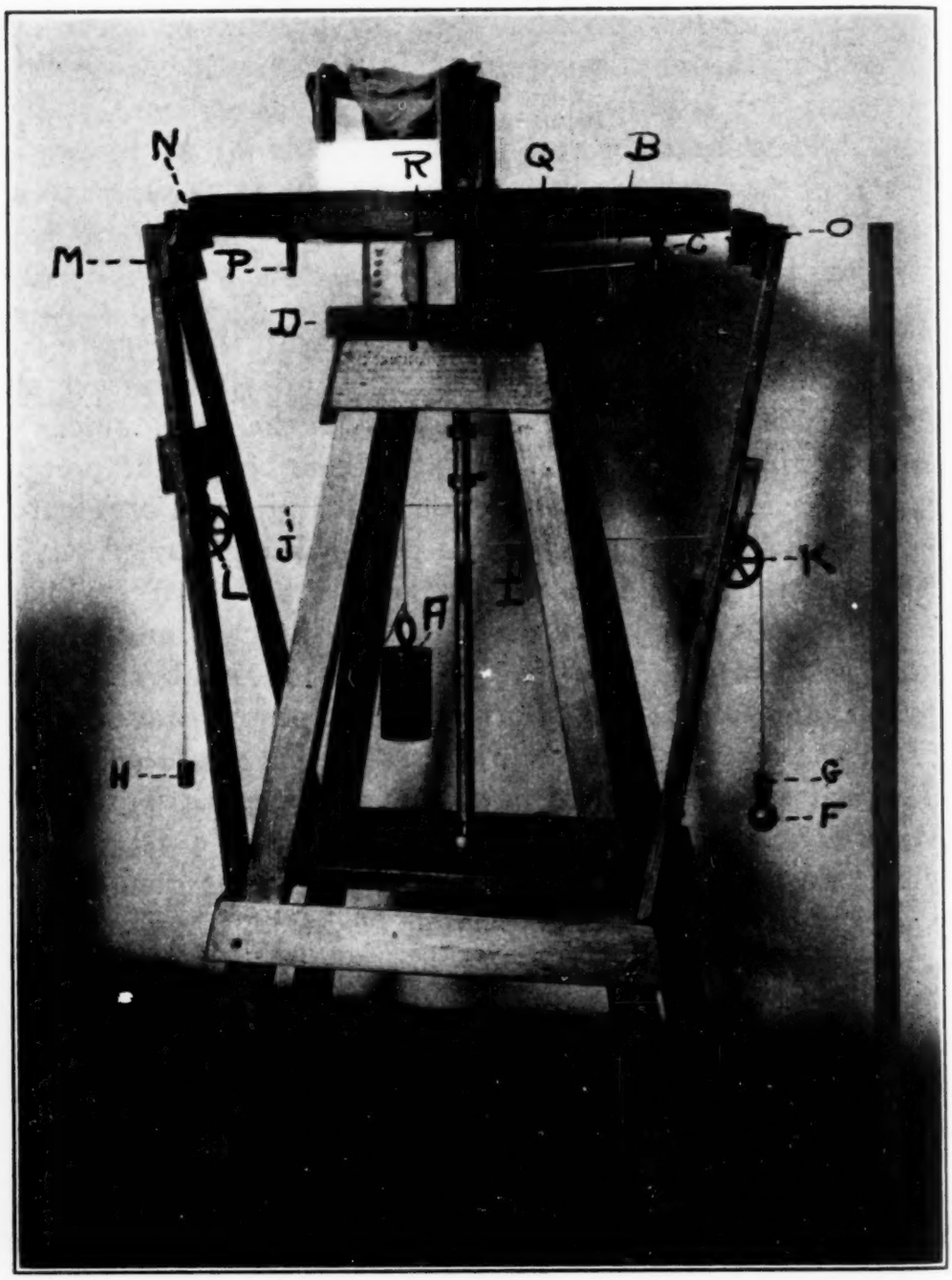
References to the accompanying figure will make clear the following description.[3] The starting mechanism consists of weight *A* which communicates the initial impulse to the table by way of the cord *B* at post *C*. By pulling down the trigger

[1] The construction of this apparatus was made possible by a grant from the Research Committee, Stanford University, with supplementary funds from the Department of Psychology.

[2] Maxwell, S. S., Burke, U. L., & Reston, C. The effect of repeated rotation on the duration of after-nystagmus in the rabbit. *Amer. J. Physiol.*, 1922, 58, 432.

[3] Our apparatus was built by Mr. Sidney W. Borrow, mechanician, Physics Department, Stanford University.

D, which engages with the under surface of the table at *R*, the operator releases the table which starts to turn under the pull of the weight. The metal ring *E* to which the cord *B* is attached drops off the post *C* as the table completes about a quarter



of a turn, *i.e.*, as soon as weight *A* has come to rest at the base of the table.

Although the amount of friction is reduced to a minimum, by reason of the fact that the axis of the table rests on a polished steel ball enclosed at *S* in a metal collar, it is still necessary to devise a means of keeping the rate of rotation constant after the initial impulse has been given to the table in the manner described. This is done by means of weight *F* which may be attached either at *G* or *H* depending on whether the direction of rotation is clockwise or counter-clockwise. As cord *I* (or *J*) unwinds over the pulley *K* (or *L*) as the table revolves, the weight *F* falls and is just sufficient to overcome the frictional resistance of the axis of the table and thus keep the rate of rotation approximately constant. The weight of *F* is 193 grams.

As weight *F* falls by reason of the unwinding of *I*, cord *J* winds around the axis of the table and *H* is pulled up to the pulley *L*. As it comes in contact with the pulley it pulls brake *M* into contact with the periphery of the revolving table at *N*, and thus stops the table. If weight *F* is attached at *H*, the other brake, *O*, is used. In this manner the table is halted without the interference of the operator and in a constant manner.

The number of revolutions of the table is dependent upon the length of cords *I* and *J*. In the present instance they allow the table to make 10 revolutions. The rate of rotation is controlled by the mass of weight *A* and distance through which it falls. In our investigations [4] the rate was 10 revolutions in 20 seconds. The weight of *A* was 2740 grams.

The direction of rotation is dependent on whether weight *A* is attached by the metal ring *E* to post *C* or post *P*. Weight *F* is attached at *G* or *H* dependent upon the direction of rotation. The releasing devise *D* is duplicated on the opposite side of the table (not visible in the picture) at *Q*. *D* or *Q* is used depending on the direction of rotation. *D* and *Q* are, of course, stationary and are mounted on wooden

[4] Fearing, F. S. Post-rotational head nystagmus in adult pigeons. *J. Comp. Psychol.*, 1926, 5, No. 1.

strips which project from the supporting platform to the periphery of the table. In order that the releasing device *D* may not interfere with the table when once it is set in motion, it pivots on an axis and is thus out of the way during the rotation of the table.

The table was calibrated by means of electrical contacts made at each quarter revolution in circuit with an electric time-marker playing on a kymograph. These calibrations showed that the acceleration took place during the first quarter-turn, after which the time per turn remained relatively constant until the last quarter-turn, when the brake was applied. At the time of calibration, when the table was set for 10 revolutions in 20 seconds, acceleration took place in the first quarter-turn which occupied about 1.48 seconds; each successive quarter-turn occupied about 0.48 second, and retardation occupied approximately 0.74 second.

The table stops smoothly and without noticeable jar to the animal. The cords are of silk permatite,[5] a material used for stringing tennis racquets. This has to be replaced occasionally. Stretching of the cord which bears weight *A* has to be guarded against. In the illustration the structural dimensions may be roughly determined by comparison with the meter stick.

The apparatus which has been described has proved satisfactory in a series of investigations covering nearly two years. It should be useful with any of the smaller laboratory animals where the period of rotation is limited.

[5] Sold by Wright and Ditson, San Francisco, California.